PHILOSOPHICAL

TRANSACTIONS

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MDCCCXXIX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion,

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as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the MEDALS for the year 1828 as follows.

A Royal Medal to Professor JOHANN FRIEDRICH ENCKE of Berlin, Foreign Member of the Royal Society, for his accurate determination of the orbit of a comet of short period, as confirmed by observation.

A Royal Medal to Dr. WILLIAM HYDE WOLLASTON, Fellow of the Royal Society, for his communication entitled "On a method of rendering Platina malleable," being the conclusion of a series of researches on the properties of the metallic bodies contained in the ores of platina.

PHILOSOPHICAL TRANSACTIONS.

I. The Bakerian Lecture.—*On a method of rendering Platina malleable.* *By* William Hyde Wollaston, *M.D. F.R.S. &c.*

Read November 20, 1828.

As, from long experience, I probably am better acquainted with the treatment of Platina, so as to render it perfectly malleable, than any other member of this Society, I will endeavour to describe, as briefly as is consistent with perspicuity, the processes which I put in practice for this purpose, during a series of years, without seeing any occasion to wish for further improvement.

The usual means of giving chemical purity to this metal, by solution in aqua regia and precipitation with sal ammoniac, are known to every chemist; but I doubt whether sufficient care is usually taken to avoid dissolving the Iridium contained in the ore, by due dilution of the solvent. In an account which I gave in the Philosophical Transactions for 1804, of a new metal, Rhodium, contained in crude platina, I have mentioned this precaution, but omitted to state to what degree the acids should be diluted. I now therefore recommend, that to every measure of the strongest muriatic acid employed, there be added an equal measure of water; and moreover, that the nitric acid used be what is called "single aquafortis;" as well for the sake of obtaining a purer result, as of economy in the purchase of nitric acid.

With regard to the proportions in which the acids are to be used, I may say, in round numbers, that muriatic acid, equivalent to 150 marble, together with nitric acid equivalent to 40 marble, will take 100 of crude platina; but in order to avoid waste of acid, and also to render the solution purer, there should be in the menstruum a redundance of 20 per cent at least of the ore.

The acids should be allowed to digest three or four days, with a heat which ought gradually to be raised. The solution, being then poured off, should be suffered to stand until a quantity of fine pulverulent ore of iridium, suspended in the liquid, has completely subsided; and should then be mixed with 41 parts of sal ammoniac, dissolved in about 5 times their weight of water. The first precipitate, which will thus be obtained, will weigh about 165 parts, and will yield about 66 parts of pure platina.

As the mother-liquor will still contain about 11 parts of platina, these, with some of the other metals yet held in solution, are to be recovered, by precipitation from the liquor with clean bars of iron, and the precipitate is to be redissolved in a proportionate quantity of aqua regia, similar in its composition to that above directed to be used: but in this case, before adding sal ammoniac, about 1 part by measure of strong muriatic acid should be mixed with 32 parts by measure of the nitro-muriatic solution, to prevent any precipitation of palladium or lead along with the ammonio-muriate of platina.

The yellow precipitate must be well washed, in order to free it from the various impurities which are known to be contained in the complicated ore in question; and must ultimately be well pressed, in order to remove the last remnant of the washings. It is next to be heated, with the utmost caution, in a black-lead pot, with so low a heat as just to expel the whole of the sal ammoniac, and to occasion the particles of platina to cohere as little as possible; for on this depends the ultimate ductility of the product.

The gray product of platina, when turned out of the crucible, if prepared with due caution, will be found lightly coherent, and must then be rubbed between the hands of the operator, in order to procure by the gentlest means, as much as can possibly be so obtained, of metallic powder, so fine as to pass through a fine lawn sieve. The coarser parts are then to be ground in a wooden bowl with a wooden pestle, but on no account with any harder material, capable of burnishing the particles of platina*; since every degree of burnishing will prevent the particles from cohering in the further stages of the process. Since the whole will require to be well washed in clean water, the

* The following experiment will prove the necessity of attending to this precaution:—if a wire of platina be divided with a sharp tool in a slanting direction, and, being then heated to redness, be struck upon an anvil with a hammer, so as to force into contact the two newly-divided surfaces, they will

operator, in the later stages of grinding, will find his work much facilitated by the addition of water, in order to remove the finer portions, as soon as they are sufficiently reduced to be suspended in it.

Those who would view this subject scientifically should here consider, that as platina cannot be fused by the utmost heat of our furnaces, and consequently cannot be freed like other metals, from its impurities, during igneous fusion, by fluxes, nor be rendered homogeneous by liquefaction, the mechanical diffusion through water should here be made to answer, as far as may be, the purposes of melting; in allowing earthy matters to come to the surface by their superior lightness, and in making the solvent powers of water effect, as far as possible, the purifying powers of borax and other fluxes in removing soluble oxides.

By repeated washing, shaking, and decanting, the finer parts of the gray powder of platina may be obtained as pure* as other metals are rendered by the various processes of ordinary metallurgy; and if now poured over, and allowed to subside in a clean basin, a uniform mud or pulp will be obtained, ready for the further process of casting.

The mould which I have used for casting, is a brass barrel, 6¾ inches long, turned rather taper within, with a view to facilitate the extraction of the ingot to be formed, being 1.12 inches in diameter at top, and 1.23 inches at a quarter of an inch from the bottom, and plugged at its larger extremity with a stopper of steel, that enters the barrel to the depth of a quarter of an inch. The inside of the mould being now well greased with a little lard, and the stopper being fitted tight into the barrel by surrounding it with blotting-paper, (for the paper facilitates the extraction of the stopper, and allows the escape of water during compression,) the barrel is to be set upright in a jug of water, and is itself to be filled with that fluid. It is next to be filled quite full with the mud of platina; which, subsiding to the bottom of the water, is sure to fill the barrel

become firmly welded together; but if the surfaces have previously been burnished with any hard substance, the welding will be effected, if at all, with very great difficulty.

When the powder of platina has been over-heated in decomposing the ammonio-muriate, or has been burnished in the grinding, I have in vain endeavoured to give it a welding surface, by steeping it in a solution of sal-ammoniac in nitric acid.

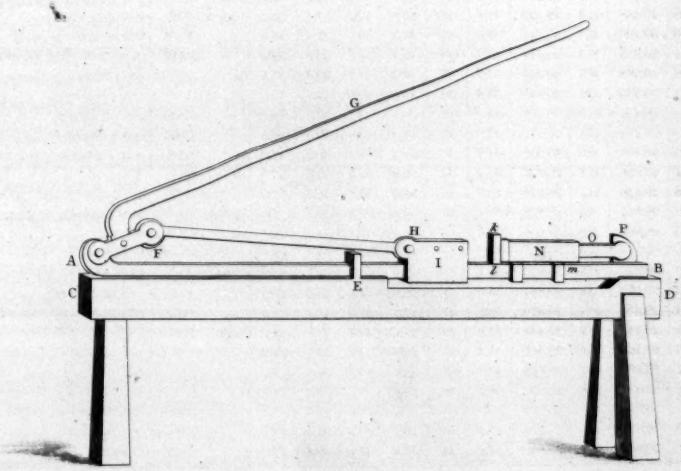
* Sulphuric acid, digested upon the gray powder of platina, thus purified, extracted less than $\frac{1}{1000}$th part of iron.

without cavities, and with uniformity,—a uniformity to be rendered perfect by subsequent pressure. In order, however, to guard effectually against cavities, the barrel may be weighed after filling it, and the actual weight of its contents being thus ascertained, may be compared with that weight of platina and water which it is known by estimate that the barrel ought to contain*. A circular piece of soft paper first, and then of woollen cloth, being laid upon the surface, allow the water to pass, during partial compression by the force of the hand with a wooden plug. A circular plate of copper is then placed upon the top, and thus sufficient consistency is given to the contents to allow of the barrel being laid horizontally in a forcible press.

The press which I have generally used for this purpose, (Plate I.), consists of a flat iron bar A B, set edgeways, and screwed down by a hook E, near its middle, where it would otherwise be liable to bend, to a strong wooden bench C D. The bar is connected by a pivot at its extremity A, with the lever A F G. An iron rod F H, which turns at its two extremities upon the pivots F and H, proceeds from the lever at F, and, as the lever descends, propells forward the carriage I, which slides along the bar. A stopper or block being placed in the vacant space I k, the carriage communicates motion to the cradle k l m, which is also made to slide along the bar, and carries the barrel N, which lies upon the cradle, straight against the piston O, which rests by its end against P, a projection in the further extremity of the bar.

The weight, which in this machine, when the angle of the lever's elevation is small, will keep the power, applied vertically at the extremity of the lever, in equilibrio = that power $\times \frac{AG \times FH}{AF\,[AF + FH]}$ $\times$ cotan. of the angle of the lever's elevation; which expression, in the case of the press actually used, becomes, Power $\times$ 5 . cotan. of the angle of the lever's elevation. This expres-

* From the mean weight of the ingots obtained in previous operations, it is known that the barrel described in the text ought to contain 16 ounces troy of dry platina powder. The weight of the contents of the barrel $= 16$ ounces $\times \frac{\text{sp. grav. of platina} - 1}{\text{sp. grav. of platina}} +$ the weight of a cubic inch of water $\times$ capacity of the barrel in cubic inches $= 16$ ounces $\times \frac{20.25}{21.25} + .526$ ounces $\times 7.05 = 18.9375$ ounces troy. Should the contents of the barrel weigh materially less than this estimated weight, there must be a want of uniformity in the disposition of the powder within the barrel.



Scale 1 inch to a Foot.

sion, at an elevation of 5°, becomes nearly 60 × power, and at an elevation of 1°, becomes nearly 300 × power; and when the lever becomes horizontal, the multiplier of the power becomes *quasi* infinite. This explanation will be sufficient to show the mechanical advantage with which, by means of this press, the weight of the operator, acting on the end of the lever, will be made to bear against the area of the section of the barrel, a circle little more than an inch in diameter.

After compression, which is to be carried to the utmost limit possible, the stopper at the extremity being taken out, the cake of platina will easily be removed, owing to the conical form of the barrel; and being now so hard and firm that it may be handled without danger of breaking, it is to be placed upon a charcoal fire, and there heated to redness, in order to drive off moisture, burn off grease, and give to it a firmer degree of cohesion.

The cake is next to be heated in a wind-furnace; and for this purpose is to be raised upon an earthen stand about 2½ inches above the grate of the furnace, the stand being strown over with a layer of clean quartzose sand, on which the cake is to be placed, standing upright on one of its ends. It is then to be covered with an inverted cylindrical pot, of the most refractory crucible ware, resting at its open end upon the layer of sand; and care is to be taken that the sides of the pot do not touch the cake.

To prevent the blistering of the platina by heat, which is the usual defect of this metal in its manufactured state, it is essential to expose the cake to the most intense heat that a wind-furnace can be made to receive, more intense than the platina can well be required to bear under any subsequent treatment; so that all impurities may be totally driven off, which any lower temperature might otherwise render volatile. The furnace is to be fed with Staffordshire coke, and the action of the fire is to be continued for about twenty minutes from the time of lighting it, a breathing heat being maintained during the last four or five minutes.

The cake is now to be removed from the furnace, and being placed upright upon an anvil, is to be struck, while hot, on the top, with a heavy hammer, so as at one heating effectually to close the metal. If in this process of forging, the cylinder should become bent, it should on no account be hammered on the side, by which treatment it would be cracked irremediably; but must be

straightened by blows upon the extremities, dexterously directed, so as to reduce to a straight line the parts which project.

The work of the operator is now so far complete, that the ingot of platina may be reduced, by the processes of heating and forging, like that of any other metal, to any form that may be required. After forging, the ingot is to be cleaned from the ferruginous scales which its surface is apt to contract in the fire, by smearing over its surface with a moistened mixture of equal parts by measure of crystallized borax and common salt of tartar, which, when in fusion, is a ready solvent of such impurities*, and then exposing it, upon a platina tray, under an inverted pot, to the heat of a wind-furnace. The ingot on being taken out of the furnace, is immediately to be plunged into dilute sulphuric acid, which in the course of a few hours will entirely dissolve the flux adhering to the surface. The ingot may then be flattened into leaf, drawn into wire, or submitted to any of the processes of which the most ductile metals are capable.

The perfection of the methods above described, for giving to platina complete malleability, will best be estimated by comparing the metal thus obtained, in respect of its specific gravity, with platina which has undergone complete fusion; and by comparing it, in respect of its tenacity, with other metals possessing that quality in the greatest perfection.

The specific gravity of platina, drawn into fine wire, from a button which had been completely fused by the late Dr. E. D. Clarke with an oxy-hydrogen blowpipe, I found to be 21.16. The aggregate specific gravity of the cake of metallic mud, when first introduced into the barrel, exclusively of moisture, is about 4.3; when taken from the press, is about 10. That of the cake fully contracted, on being taken out of the wind-furnace before forging, is from 17 to 17.7. The mean specific gravity of the platina, after forging, is about 21.25, although that of some rods, after being drawn, is 21.4: but that of fine platina

* The chemist will find this flux very serviceable for removing from his crucible or other vessels of platina those ferruginous scales with which, after long use, and particularly after being strongly heated in a coal or coke fire, they become incrusted. In the analysis of earthy minerals, I have been in the habit of using a similar flux, composed of 2 parts by weight of crystallized carbonate of soda, and 1 of crystallized borax, well ground together. It has the advantage of not acting, like caustic alkali, upon the platina crucible, and is a powerful solvent of jargon and many other minerals, which yield with difficulty to other fluxes. If the mineral to be operated on requires oxidation, in order to decompose it, a little nitre or nitrate of soda may be added.

wire, determined by comparing the weight of a given length of it with the weight of an equal length of gold wire drawn through the same hole, I find to be 21.5, which is the maximum specific gravity that we can well expect to be given to platina.

The mean tenacity, determined by the weights required to break them, of two fine platina wires, the one of $\frac{1}{3000}$, the other of $\frac{1}{3850}$ of an inch in diameter, reduced to the standard of a wire $\frac{1}{10}$th of an inch in diameter, I found to be 409 pounds; and the mean tenacity of 11 wires, beginning with $\frac{1}{3300}$ and ending with $\frac{1}{33.000}$ of an inch, reduced to the former standard, I found to be 589 pounds; the maximum of these 11 cases being 645 pounds, and the minimum 480 pounds. The coarsest and the finest wire which I tried, present exceptions, since a wire of $\frac{1}{1300}$ of an inch gave 290 pounds, and a wire of $\frac{1}{30.000}$ of an inch, 190 pounds. If we take 590 pounds, as determined by the 11 consecutive trials, to be the measure of the tenacity of the platina prepared by the processes above described, and consider that the tenacity of gold wire, reduced to the same standard, is about 500, and that of iron-wire, 600, we shall have full reason to be satisfied with the processes, detailed in the present paper, by which Platina has been rendered malleable.

To this paper I beg to subjoin an account of some processes relating to two of the metals which are found in the ore of platina.

To obtain malleable Palladium, the residuum obtained from burning the prussiate of that metal is to be combined with sulphur, and each cake of the sulphuret, after being fused, is to be finally purified by cupellation, in an open crucible, with borax and a little nitre. The sulphuret is then to be roasted, at a low red heat, on a flat brick, and pressed, when reduced to a pasty consistence, into a square or oblong and perfectly flat cake. It is again to be roasted very patiently, at a low red heat, until it becomes spongy on the surface. During this process, sulphur flies off in the state of sulphurous acid, especially at those moments when the heat is allowed occasionally to subside. The ingot is then to be cooled; and when quite cold, is to be tapped with a light hammer, in order to condense and beat down the spongy excrescences on its surface. The alternate roastings and tappings (or gentle hammerings) require the utmost

patience and perseverance, before the cake can be brought to bear hard blows: but it may, by these means, at length be made so flat and square, as to bear being passed through the flatting-mill, and so laminated to any required degree of thinness.

Thus prepared, it is always brittle, while hot; possibly, from its still containing a small remnant of sulphur. I have also fused some palladium per se, without using sulphur; but I have always found it, when treated in this way, so hard and difficult to manage, that I greatly prefer the former process.

To obtain the oxide of Osmium in a pure, solid, and crystallized state, I grind together, and introduce, when ground, into a cold crucible, 3 parts by weight of the pulverulent ore of iridium, and 1 part of nitre. The crucible is to be heated to a good red in an open fire, until the ingredients are reduced to a pasty state; when osmic fumes will be found to arise from it. The soluble parts of the mixture are then to be dissolved in the smallest quantity of water necessary for the purpose, and the liquor, thus obtained, is to be mixed, in a retort, with so much sulphuric acid, diluted with its weight of water, as is equivalent to the potash contained in the nitre employed; but no inconvenience will result from using an excess of sulphuric acid. By distilling rapidly into a clean receiver, for so long a time as the osmic fumes continue to come over, the oxide will be collected in the form of a white crust on the sides of the receiver; and there melting, it will run down in drops beneath the watery solution, forming a fluid flattened globule at the bottom. When the receiver has become quite cold, the oxide will become solid and crystallize. One such operation has yielded 30 grains of the crystallized oxide, besides a strong aqueous solution of it.

II. *A description of a microscopic doublet.* By WILLIAM HYDE WOLLASTON, *M.D. F.R.S. &c.*

Read November 27, 1828.

THE state of my health induces me to commit to writing, rather more hastily than I have been accustomed to do, some observations on microscopes; and I trust, that in laying them before the Royal Society, they will meet with that indulgence which has been extended to all my former communications.

In the illumination of microscopic objects, whatever light is collected and brought to the eye, beyond that which is fully commanded by the object-glasses, tends rather to impede than to assist distinct vision.

My endeavour has been, to collect as much of the admitted light as can be done by simple means, to a focus in the same plane as the object to be examined. For this purpose I have used with success a plane mirror to direct the light, and a plano-convex lens to collect it; the plane side of the lens being towards the object to be illuminated.

With respect to the apparatus for magnifying, notwithstanding the great improvements lately made in the construction of microscopes, by the introduction of achromatic object-glasses, and the manifest superiority they possess over any single microscope, in the greater extent of field they present to view at once, whereby they are admirably adapted to make an entertaining exhibition of known objects, hardly any one of the compound microscopes which I have yet seen, is capable of exhibiting minute bodies with that extreme distinctness which is to be attained by more simple means, and which is absolutely necessary for an original examination of unknown objects.

My experience has led me to prefer a lens of a plano-convex form, even when made of glass; but the sapphire lens of this form, recently introduced into use by Mr. PRITCHARD, has a decided superiority over every single lens hitherto employed.

The cost, however, of such a lens in comparison with glass, as well as the readiness with which any number and variety of the latter kind can be procured, led me to consider what simple combinations of them might perhaps equal the sapphire lens in performance, without great cost, or difficulty of construction; and though both Mr. HERSCHEL and Professor AIRY have recently applied their superior talents to the analytical investigation of this subject, it seemed not impossible that the more humble efforts of a mere experimentalist, might be rewarded by some useful results.

The consideration of that form of eye-piece for astronomical telescopes called Huygenian, suggested the probability that a similar combination should have a similar advantage, of correcting both chromatic and spherical aberration, if employed in an opposite direction as a microscope.

The construction which I found convenient in my trials, may be not unaptly compared to two thimbles fitted one within the other by screwing, and each perforated at the extremity. By this construction, two suitable plano-convex lenses fixed in these perforations, may, because of their plane surfaces, have their axes easily placed in the same line; and their distance from each other may be so varied, by screwing, as to produce the best effect of which they are susceptible.

As far as my trials have hitherto gone, I am led to consider the proportion of 3 to 1 as nearly the best for the relation of the foci of these lenses; and their joint performance to be the most perfect, when the distance between their plane surfaces is about $1\frac{4}{10}$ of the shorter focus. But as all the lenses I possess are not similar segments of spheres, or of the same relative thickness, I could not expect exact uniformity in the results.

The following is a description of the apparatus which I have employed.

T, U, B, E, (Plate II. fig. 1.) represents a tube about six inches long, and of such a diameter as to preclude any reflexion of false light from its sides; and the better to insure this, the inside of the tube should be blackened. At the top of the tube, or within it, at a small distance from the top, is placed either a plano-convex lens E, T, or one properly crossed, so as to have the least aberration, about three-quarters of an inch focus, having its plane side next the object to be viewed; and at the bottom is a circular perforation A, of about three-tenths of an inch diameter, for limiting the light reflected from the plane

Fig. 1.

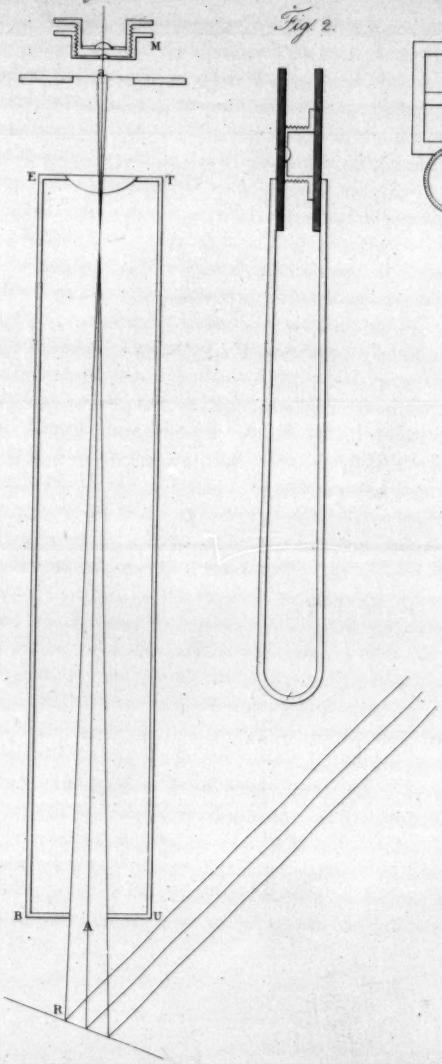
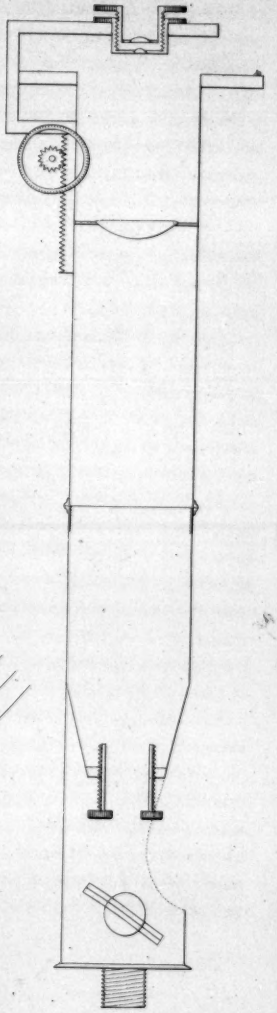


Fig. 2.

Fig. 3.



mirror R, and which is to be brought to a focus at *a*, giving a neat image of the perforation A at the distance of about eight tenths of an inch from the lens E, T, and in the same plane as the object which is to be examined. The length of the tube and the distance of the convex lens from the perforation may be somewhat varied. The length here given, six inches, being that which it was thought would be most convenient for the height of the eye above the table. The diameter of the image of the perforation A, need not, excepting with lower powers than are here meant to be considered, exceed one-twentieth of an inch.

The intensity of illumination will depend upon the diameter of the illuminating lens, and the proportion of the image to the perforation, and may be regulated according to the wish of the observer.

The compound magnifier M, consists, as before mentioned, of two plano-convex lenses; the proportion of the foci of these lenses being about as 3 to 1. They are fixed in their cells, having their plane sides next to the object to be viewed, their plane surfaces being distant from each other about $1\frac{4}{10}$ or $1\frac{1}{2}$ of the length of the shorter focus. This distance should be varied by trial, until the utmost possible degree of distinctness has been attained, not only in the centre, but throughout the whole field of view.

In order to determine the distance between the plane surfaces of the lenses, I have used the following contrivance. A wire (Plate II. fig. 2.) is bent so as to form a spring, to the ends of which two small pieces of plane glass are attached. Between the surfaces of the pieces of glass is placed, in the manner represented in the plate, the interior cell, or that which carries the lens of the longer focus; and the distance between the exterior surfaces of the pieces of glass is to be measured with a pair of callipers: the cell is then to be screwed into its place, and the compound cell subjected to the same operation; when the increase of distance between the exterior surfaces of the pieces of glass will evidently be equal to the distance between the plane surfaces of the lenses.

The exterior cell of the compound magnifier should be formed with a flanch, so that it may rest upon the piece that receives it. This is a far more convenient method than screwing, and the magnifiers can be more readily changed.

The lens E, T, or the perforation A, should have an adjustment by which the distance between them may be varied, and the image of the perforation be thus

brought into the same plane as the object to be examined. This may perhaps be most conveniently done by two tubes screwing one into the other.

A stage for carrying the object, furnished with the requisite means for lateral adjustments, is fixed at *a*, between the magnifier and the lens E, T. The adjustment for distinct vision is applied to the piece carrying the compound magnifier.

For the perfect performance of this microscope, it is necessary that the axes of the lenses and the centre of the perforation A, should be in the same right line. This may be known by the image of the perforation being illuminated throughout its whole extent, and having its whole circumference equally well defined. For illumination at night, a common bull's-eye lanthorn may be used with great advantage.

With this microscopic doublet I have seen the finest striæ and serratures upon the scales of the Lepisma and Podura, and the scales upon a gnat's wing, with a degree of delicate perspicuity which I have in vain sought in any other microscope with which I am acquainted.

Before I conclude, I would point out one great advantage that has confirmed me in the preference I have given to the use of a plano-convex lens, properly employed; that is, having its plane side next to the object: namely, that if such a lens should touch a fluid under examination, the view is not only not impaired, but even improved by the contact of the two media; but if a double convex lens be used, and it should accidentally touch the fluid, which not unfrequently happens when the lens is of short focus, there is an end of the examination, until the lens has been taken out, wiped, and replaced.

London,
October 28th, 1828.

Appendix.

The instrument which has been described will of course admit of many varieties of form; I shall, however, add a description of that which has appeared to me to be convenient, and which is represented at Plate II. fig. 3. A tube of sufficient length and diameter forms the body of the instrument; one

end of the tube is closed by a piece having a screw, by means of which it may be fixed in the top of the box intended to contain the instrument, which thus forms a support. A portion of the tube above this piece is cut away, as marked by the dotted line, for the purpose of admitting light to the small mirror which is attached to an horizontal axis passing through the diameter of the tube. The inclination of this mirror may be varied by means of a milled head fixed to the axis on the outside of the tube; the other adjustment at right angles being made by turning the box of the microscope.

Into the tube above the opening a conical piece is soldered, into which is screwed a small cylindrical tube carrying the perforation before described. The plano-convex lens is fixed in a spring tube, which slides into that which forms the body of the microscope. The position, consequently, of the lens may be varied so as to bring the image of the perforation into the same plane with the object to be viewed. A piece of plate glass about two inches square, or less if it be thought more convenient, is attached to the top of the tube, and serves to support a stage having lateral adjustments at right angles to each other. The piece into which the magnifiers fit, may be moved by a rack and pinion, and great care must be taken to arrange this adjustment, so that the magnifier may move precisely in the prolongation of the axis of the tube. The tube is divided into two pieces, of equal lengths, which screw into each other, and which when taken asunder will allow of the whole instrument being packed in a box about four inches square.

Supposing the plano-convex lens to be placed at its proper distance from the stage, the image of the perforation may be readily brought into the same plane with the object, by fixing temporarily a small wire across the perforation with a bit of wax, viewing any object placed upon a piece of glass upon the stage of the microscope, and varying the distance of the perforation from the lens by screwing its tube until the image of the wire is seen distinctly at the same time with the object upon the piece of glass.

III. *An account of some experiments on the Torpedo.* *By Sir* HUMPHRY DAVY, *Bart. F.R.S.*

Read November 20, 1828.

AMIDST the variety of researches which have been pursued respecting the different forms and modes of excitation and action of electricity, it is surprising to me that the electricity of living animals has not been more an object of attention, both on account of its physiological importance, and its general relation to the science of electro-chemistry.

In reading an account of the experiments of WALSH, it is impossible not to be struck by some peculiarities of the electricity of the organ of the Torpedo and Gymnotus; such as its want of power to pass through air, and the slight effects of ignition produced by the strongest shocks: and though Mr. CAVENDISH, with his usual sagacity, compared its action to that of a battery weakly charged, when the electricity was large in quantity but low in intensity, yet the peculiarities which I have just mentioned are not entirely in harmony with this view of the subject.

When VOLTA discovered his wonderful pile, he imagined he had made a perfect resemblance of the organ of the Gymnotus and Torpedo; and whoever has felt the shocks of the natural and artificial instruments, must have been convinced, as far as sensation is concerned, of their strict analogy. After the discovery of the chemical power of the Voltaic instrument, I was desirous of ascertaining if this property of electricity was possessed by the electrical organs of living animals; and being in 1814 and 1815 on the coast of the Mediterranean, I made use of the opportunities which offered themselves of making experiments on this subject. Having obtained in the Bay of Naples, in May 1815, two small Torpedos alive, I passed the shocks through the interrupted circuit made by silver wire through water, without being able to perceive the slightest decomposition of that fluid; and I repeated the same experiments at

Mola di Gaeta, with an apparatus in which the smallest possible surface of silver was exposed, and in which good conductors, such as solutions of potassa and sulphuric acid, were made to connect the circuit; but with the same negative results.

Having obtained a larger Torpedo at Rimini in June in the same year, I repeated the experiments, using all the precautions I could imagine, with like results; and at the same time I passed the shock through a very small circuit, which was completed by a quarter of an inch of extremely fine silver wire, drawn by the late Mr. CAVENDISH for using in a micrometer, and which was less than the $\frac{1}{1000}$dth of an inch in diameter; but no ignition of the wire took place. It appeared to me after these experiments, that the comparison of the organ of the Torpedo to an electrical battery weakly charged, and of which the charged surfaces were imperfect conductors, such as water, was more correct than that of the comparison to the pile: but on mentioning my researches to Signor VOLTA, with whom I passed some time at Milan that summer, he showed me another form of his instrument, which appeared to him to fulfill the conditions of the organs of the torpedo; a pile, of which the fluid substance was a very imperfect conductor, such as honey or a strong saccharine extract, which required a certain time to become charged, and which did not decompose water, though when charged it communicated weak shocks.

The discovery of ŒRSTED of the effects of Voltaic electricity on the magnetic needle, made me desirous to ascertain if the electricity of living animals possessed this power; and after several vain attempts to procure living torpedos sufficiently strong and vigorous to give powerful shocks, I succeeded in October of this year, through the kind assistance of GEORGE DURING, Esq., His Majesty's Consul at Trieste, in obtaining two lively and recently caught Torpedos, one a foot long, the other smaller. I passed the shocks from the largest of these animals a number of times through the circuit of an extremely delicate magnetic electrometer, (of the same kind, but more sensible, than that I have described in my last paper on the electro-chemical phænomena, which the Royal Society has honoured with a place in their Transactions for 1826,) but without perceiving the slightest deviation of or effect on the needle; and I convinced myself that the circuit was perfect, by making my body several times a part of it, holding the silver spoon, by which the shock was taken, in one hand,

wetted in salt and water, and keeping the wire connected with the electrometer in the other wet hand; the shocks which passed through the reduplications of the electrometer were sufficiently powerful to be felt in both elbows, and once even in the shoulders.

These negative results may be explained by supposing that the motion of the electricity in the torpedinal organ is in no measurable time, and that a current of some continuance is necessary to produce the deviation of the magnetic needle; and I found that the magnetic electrometer was equally insensible to the weak discharge of a Leyden jar as to that of the torpedinal organ; though whenever there was a continuous current from the smallest surfaces in Voltaic combinations of the weakest power, but in which some chemical action was going on, it was instantly and powerfully affected. Two series of zinc and silver, and paper moistened in salt and water, caused the permanent deviation of the needle several degrees, though the plates of zinc were only $\frac{1}{2}$th of an inch in diameter.

It would be desireable to pursue these inquiries with the electricity of the Gymnotus, which is so much more powerful than that of the Torpedo: but if they are now to be reasoned upon, they seem to show a stronger analogy between common and animal electricity, than between voltaic and animal electricity: it is however I think more probable that animal electricity will be found of a distinctive and peculiar kind.

Common electricity is excited upon non-conductors, and is readily carried off by conductors and imperfect conductors. Voltaic electricity is excited upon combinations of perfect and imperfect conductors, and is only transmitted by perfect conductors or imperfect conductors of the best kind.

Magnetism, if it be a form of electricity, belongs only to perfect conductors; and, in its modifications, to a peculiar class of them.

The animal electricity resides only in the imperfect conductors forming the organs of living animals, and its object in the œconomy of nature is to act on living animals.

Distinctions might be established in pursuing the various modifications or properties of electricity in these different forms; but it is scarcely possible to avoid being struck by another relation of this subject. The torpedinal organ depends for its powers upon the will of the animal. John Hunter has shown

how copiously it is furnished with nerves. In examining the columnar structure of the organ of the Torpedo, I have never been able to discover arrangements of different conductors similar to those in galvanic combinations, and it seems not improbable that the shock depends upon some property developed by the action of the nerves.

To attempt to reason upon any phænomena of this kind as dependent upon a specific fluid, would be wholly vain.

Little as we know of the nature of electrical action, we are still more ignorant of the nature of the functions of the nerves. There seems, however, a gleam of light worth pursuing in the peculiarities of animal electricity, its connection with so large a nervous system, its dependence upon the will of the animal, and the instantaneous nature of its transfer, which may lead when pursued by adequate inquirers to results important for physiology.

The weak state of my health will, I fear, prevent me from following this subject with the attention it seems to deserve; and I communicate these imperfect trials to the Royal Society, in the hope that they may lead to more extensive and profound researches.

October 24th, 1828.
Lubiana, Illyria.

IV.—*On a method of comparing the light of the sun with that of the fixed stars.*

By William Hyde Wollaston, *M.D. F.R.S.*

Read December 11, 1828.

ONE of the most ingenious contributors to the Transactions of our Society in the last century, the Rev. John Michell, in a paper intituled "An inquiry into the probable parallax and magnitude of fixed stars, &c*." has proposed it to astronomers, as an object worthy their attention, to determine what proportion the light, afforded us separately by each fixed star, bears to the light which we receive from the sun; since, from our inability to measure the annual parallax of those very remote bodies, such a comparison is the best, perhaps the only method within our reach, of obtaining, though not certain, yet probable estimates of their distances; and thus forming reasonable conjectures concerning the extent of the visible universe. In order that we may judge, with the least chance of error, of the mean distance of those stars which are the nearest to the earth, he directs us to compare the light of the brightest stars with that of the sun, and next to calculate how far the sun must be removed, to make the light that we should then receive from him, not more than equal to the mean light of the stars chosen for comparison.

Mr. Michell made, as he says, some rude experiments for determining the comparative brilliancy of certain principal stars; but has not suggested any contrivance for comparing a star with the sun. He states, however, so distinctly the great object of such a comparison, and the inferences which an industrious observer would thence be entitled to draw, concerning the distances of those stars whose light he might succeed in measuring, that it is surprising that no astronomer has been incited by these remarks to devise a method of making the requisite observations, and that now, so many years after Mr. Michell's suggestion was made public, so much remains to be effected in this branch of photometry.

* Phil. Trans. 1767: p. 234.

From a comparison which I made in the year 1799 (by a method described in the note subjoined) of the light of the sun with that of the moon, I should estimate the direct light of the sun as being nearly one million times greater than that of the moon*; and consequently the direct light of the sun as very many millions times greater than that afforded us by all the fixed stars, taken collectively. Such then being, to our visual organs, the vast disproportion in radiance between the sun and the whole starry firmament, it is not to be expected that we should assign very accurately how much greater the light of

* The observations on which this estimate is founded, are given in detail at the end of the Appendix to this paper. The mode of making the observations was the following.

The sun's light was compared with that of a candle, by admitting a beam of it into a room through a small circular hole in a plate of metal, fastened in a window-shutter; and a small cylinder of any opaque material being placed in the beam, so as to cast a shadow upon a screen, the distance of a candle from the same cylinder (or an equal one placed at the same distance from the screen) was varied, until the shadow in the line of the candle became equally intense with the shadow in the line of the sun. The direct light of the moon was compared with the light of a candle in the same manner. This method of comparing lights by the intensity of the shadows which they occasion, was pursued also by Count Rumford.

It appears from the mean of the observations given in No. V. of the Appendix, that the light of the sun is equal to that of 5563 candles placed at the distance of one foot; a result which accords very nearly with that of Bouguer. For he states the light of the sun to be equal to that of 11,664 wax candles at the distance of 16 inches French, which is equivalent to 5774 wax candles at the distance of one foot English. It appears also from my experiments, that the light of the full moon is equal to $\frac{1}{144}$th part of the light of a candle, placed at the distance of a foot; and hence, that the sun's light is equal to $5563 \times 144 \times$ moon's light $= 801{,}072. \times$ moon's light. Bouguer, who differs greatly from me in the comparison of the moon with a candle, states the light of the sun to be $= 300{,}000 \times$ moon's light. The proportion which the light of the full moon ought to bear to the light of the sun, on the supposition that the moon gives off again all the solar light that falls upon it, has been differently estimated by several mathematicians who have computed it. The light of the sun at the earth being represented by unity, Mr. Michell expresses that of the full moon by $\sin^2 \frac{1}{2}$ (diameter $= \frac{1}{465{,}000}$. Euler, in the Transactions of the Berlin Academy for 1750, represents the light of the full moon by $\frac{1}{2} \sin^2 \frac{1}{2}$ (diameter, which is only $\frac{1}{3}$th of the former expression of Mr. Michell. Neither of these expressions, however, appears to be correct. For if we consider that the quantity of solar light which falls upon any point in the moon's surface, must vary, if we regard the sun's rays as parallel, as the cosine of the angular distance of that point from the point in the moon over which the sun is vertical, we shall obtain, by following Euler's own method, the formula $\frac{1 + 2 \sin^3 (\text{ semidiameter} - \cos^3 (\text{ semidiameter}}{3}$, to express the quantity of light, which, on the given supposition, we ought to receive from the moon; and this expression reduced to numbers $= \frac{1}{100{,}000}$. The moon therefore appears to give off only about $\frac{1}{3}$th of the light which she receives.

the sun is, than that exceedingly minute quantity of it which shines upon us from any one, even the most brilliant of the fixed stars.

It may be remembered that on a former occasion, in examining the correct performance of a good telescope, I found that the sun's image, reflected from the surface of a small sphere, (such as that of a thermometer-bulb filled with mercury,) and viewed at a proper distance through a telescope, is, to appearance, extremely like a fixed star, and forms, in such experiments, an admirable substitute for one, in being really fixed, and therefore well adapted for deliberate observation. It occurred to me, while engaged in this examination, that by comparing such a reflected image with one of the larger stars, I might be able to obtain some grounds for estimating the light of the star.

It would be desireable, though extremely difficult, in conducting such an experiment, to make a direct comparison between the star and the sun's image; since in that case we should be enabled to avoid the uncertainties inseparable from an indirect comparison, the consequence of observing at times so distant, that the atmosphere in the interval has undergone considerable change. As, however, the only practicable method of observing is the indirect one, by comparing the two objects with some common standard at different times, we must endeavour to remove those uncertainties from our results, by repeating each series of comparisons so frequently, that the average of each series may be affected by atmospheric vicissitudes, or may fairly be presumed to be so, in an equal degree.

The common standard of comparison which I chose, was the image of a candle, reflected from a small thermometer-bulb, (in most trials about $\frac{1}{4}$th of an inch in diameter,) filled with mercury, and seen by one eye through a lens of about two inches focus, at the same time that the sun's image, reflected (in the manner above described) from a thermometer-bulb placed at a distance, or the star itself, was viewed by the other eye through a telescope.

In order to make the light of the two objects, when seen through the telescope, and that of the candle, more nearly alike in colour, I placed two yellow glasses at the eye-piece; and I thought it expedient to have in view, at the same time with the subject of comparison, two candles, one of tallow, the other of wax; that by making the star, or the little sun, a mean between the

two lights, I might obtain a nearer approximation to the truth*. The measure taken in each experiment was the distance of the two candles from the bulb; and every distance that I have reported amongst the observations, was the mean result of several trials.

In reducing these observations we have to consider that though the image of the sun, which is half the radius distant from the centre of the bulb, subtends at its surface the same angle, of half a degree, as the sun itself, and therefore to an eye placed at the surface would appear equally brilliant with the sun itself; yet the apparent diameter of this little sun will decrease in proportion as the eye recedes from the bulb, so that at the distance of D inches, the apparent diameter of the image will be reduced in the ratio of $\frac{1}{4}$th of the diameter of the bulb, or of $\frac{B}{4}$, to D, and consequently the brightness of the image will be reduced in the ratio of 1 to the square of $\frac{4\,D}{B}$.

If the distance of the eye from the bulb be so chosen, that, on comparing the little sun and the star, separately, with the candle's image, the candle in the two cases is at unequal distances from its bulb, d being made to represent the candle's distance from the bulb in comparing it with the sun, and δ the candle's distance from the bulb in comparing it with the star, $\frac{4\,D}{B} \times \frac{\delta}{d}$ will be the distance at which the little sun would appear of equal brightness with the star, and the brightness of the little sun would then be to the brightness of the sun itself as 1 to $\left[\frac{4\,D \times \delta}{B \times d}\right]^2$.

If, in two comparisons made, the one between the candle and the sun, the other between the candle and a star, the candle be reflected by bulbs of different diameters, and viewed with lenses of unequal focal length, the apparent diameter of the candle's image will be as the diameter of the bulb directly, and as the focal length of the lens inversely; and hence, if b be the diameter of the bulb and l the focal length of the lens in comparing the candle with the sun, and β be the diameter of the bulb and λ the focal length of the lens in com-

* If any other artificial light could be found, which would at all times be of uniform brilliancy, and of so white a colour as to supersede the necessity of using yellow glasses, it would of course be preferable, as a standard, to the light of a candle.

paring the candle with the star, $\frac{4D}{B} \times \frac{\delta}{d} \times \frac{\lambda}{l} \times \frac{b}{\beta}$ will be the distance at which the little sun would appear of equal brightness with the star; and the brightness of the little sun would then be to the brightness of the sun itself as 1 to $\left[\frac{4D \times \delta \times \lambda \times b}{B \times d \times l \times \beta}\right]^2$; and it is according to the latter formula that the observations, made with bulbs of different diameter and with lenses of different focal length, have been reduced so as to be compared in No IV. of the Appendix.

The first star that I compared with the sun, was Sirius; and the observations were made at times when, the altitudes of the two bodies being not very widely different, their powers of illumination might be presumed to be affected, on the average, in almost an equal degree by the atmosphere. The table of reduced observations [No. IV. of the Appendix], in which each of seven observations of the sun is compared with each of seven observations of Sirius, will be found to exhibit discordances, which are referrible, probably, to our variable climate, and to the smoky atmosphere of London. Uniformly transparent skies are requisite to give uniformity to such experiments; and in our climate, therefore, though the mean of very many comparisons would, probably, give a result not very remote from the mean of a much smaller number of trials made under a less variable atmosphere, we must expect the greatest and least results to differ widely from one another*.

The mean of the various trials seems to show, that the light of Sirius is equal to that of the sun reflected from the surface of a sphere $\frac{1}{10}$th of an inch in diameter, and seen at the distance of about 210 feet. The diameter of such an image of the sun, is to that of the sun itself as 1 to 100,000; and, consequently, the brightness of the image would be to the brightness of the sun itself as 1 to 10,000,000,000; but as nearly half of the light must be lost during reflection, we are not warranted by these experiments in supposing that the light of Sirius exceeds a 20,000,000,000th part of the sun's light.

* An observer, intending to pursue this inquiry, would do well, therefore, to choose a favourable climate; and, further, he ought to select such stars for comparing with the sun, as have, severally, at the times of observation, nearly the same altitude with the sun. The accuracy of these comparisons with the sun would admit of rigorous investigation, by comparing the same stars with one another. Stars having the same R might be compared at places having different latitudes, or even in different hemispheres, whereby the unequal influence of the atmosphere at different altitudes might be wholly eliminated.

Were the sun removed to such a distance, that the light which we received from it were only a twenty thousand millionth part of its present light, that distance would be equal to $\sqrt{2} \times 100{,}000 \times$ its present distance, and it would, if still situated in the ecliptic, have a parallax in longitude of nearly 3″; but if placed at the same angular distance from the ecliptic, as Sirius, since the parallax varies as the sine of a star's latitude, and the latitude of Sirius is about 39°½, it would have a parallax in latitude of about $1''\frac{9}{10}$.

Assuming the parallax of Sirius to be half a second, and consequently its distance from the earth to be 525,481 times the distance of the sun from the earth, Sirius, if placed at the sun's distance, would subtend 3.7 times the sun's apparent diameter, and would afford us as much light as 13.8 suns.

From similar experiments to those I made on Sirius, it appeared that the light afforded us by Lyra was about $\frac{1}{180{,}000{,}000{,}000}$th part of the sun's light, or about $\frac{1}{13}$th part of the light of Sirius.

Without extending this method to a comparison of the stars with the sun, we may confine it, if we think proper, to comparing the stars with one another, so that, in fulfilment of the wishes of Mr. MICHELL*, "instead of distributing them, as has hitherto been done, into a few ill-defined classes, they may be ranked with precision, both according to their respective brightness, and the exact degree of it."

In concluding the paper which is now submitted to the Society, I request them to direct their attention rather to the method than to the observations; for these have been much too few in number to enable me to state with any degree of confidence, what proportion the light of the sun really does bear to either of the stars compared with it. It was my intention, had my health permitted it, to have proceeded with this inquiry, until by multiplied observations I had ascertained how nearly the mean of one extensive series of comparisons accorded with the mean of another series; and how far, therefore, the method itself was deserving of confidence. But since I have now no prospect of bringing the subject to perfection, I submit the method itself to the consideration of industrious observers, who will soon be able to judge of the expediency of continuing the inquiry.

* Phil. Trans. 1767: p. 241.

Appendix.

I. Observations of a reflected image of the Sun, compared with a similar image of a Candle.

The sun's image, reflected from a thermometer-bulb filled with mercury, is viewed from a distance by the observer through a telescope, with power 36, and two yellow glasses at the eye-piece. The image of a candle, reflected from a similar bulb, is viewed through a lens of from 2 to 2½ inches focus; and the distance of the candle is varied, until its image appears of equal brightness with the image of the sun.

Date of the Observation.	B Diameter of the thermometer-bulb, for ☉, in inches.	D Distance of the thermometer-bulb, for ☉, in inches.	b Diameter of the thermometer-bulb, for Candle, in inches.	d Distance of the thermometer-bulb, from the Candle, in inches.	l Focus of Lens for viewing the image of Candle, in inches.
1826, March 10	.19	1440	.44	68	2.0
1827, March 14	.26	2928	.26	42	2.5
.... March 16	.26	2928	.26	28	2.5
.... March 16[a]	.11	1440	.26	41	2.5
.... March 25	.26	2928	.26	36	2.5
.... March 25[b]	.11	1440	.26	57	2.5
.... April. 6	.11	1440	.26	49	2.5

II. Observations of Sirius, compared with a reflected image of a Candle.

Sirius is viewed by the observer through a telescope, with power 36, and two yellow glasses at the eye-piece. The image of a candle, reflected from a thermometer-bulb filled with mercury, is viewed through a lens of from 2 to 2½ inches focus; and the distance of the candle is varied, until its image appears of equal brightness with that of Sirius.

Date of the Observation.	β Diameter of the Thermometer-bulb, for Candle, in inches.	δ Distance of the Thermometer-bulb, from the Candle, in inches.	λ Focus of Lens for viewing the image of the Candle, in inches.	Remarks.
1826, March 15	.44	216	2.0	
.... March 19	.44	165	2.0	Very bright night.
1827, Feb[y]. 14	.44	246	2.0	
.... Feb[y]. 15	.44	170	2.0	Very bright night, 10[h] 30[m].
.... March 14	.26	102	2.5	
.... April. 4	.26	90	2.5	
.... April. 9	.26	93	2.5	7[h] 15[m].

III. Observations of α Lyræ, compared with a reflected image of a Candle.

1827, April 9	.26	276	2.5	

IV. Reduction of the preceding observations of the Sun and Sirius.

If all the light of the sun, which falls on the thermometer-bulb, be reflected by it, the light of $\odot = \left[\frac{4D \times \delta \times \delta \times \lambda}{B \times \beta \times d \times l}\right]^2 \times$ the light of Sirius; and since there are seven observations of the sun compared with a candle, and seven of Sirius, there will be forty-nine different values of the expression $\frac{4D \times \delta \times \delta \times \lambda}{B \times \beta \times d \times l}$; which are all inserted in the following Table.

Observations of ⊙.	1826. March 10.	1827. March 2.	1827. March 16.	1827. March 16².	1827. March 25.	1827. March 25².	1827. April 6.	Totals.
Of Sirius.								
1826, March 15	96.297	107.022	160.533	127.441	124.859	91.668	106.634	814.454
.... March 19	73.560	81.732	122.629	97.351	95.398	70.024	81.458	622.152
1827, Feb' 14	109.672	121.886	182.829	145.141	142.200	104.400	121.725	927.853
.... Feb' 15	75.789	84.230	126.345	100.301	98.268	72.146	83.925	641.004
.... March 14	98.435	109.397	164.096	127.630	130.270	93.703	109.002	832.533
.... April 4	86.854	96.527	144.791	112.625	114.944	82.679	96.178	734.598
.... April 9	89.749	99.745	149.622	116.366	118.776	85.435	99.384	759.077
								5.331.671

$$\frac{5.331.671}{49} = 108.809$$

Hence the mean result of the foregoing experiments is that, supposing none of the Sun's light to be lost on reflection at the thermometer-bulb,

$$\odot\text{'s light} = 108.809^2 \times \text{light of Sirius}$$
$$= 11.839.533.000 \times \text{the light of Sirius};$$

but, allowing for the loss of nearly half the light on reflection, that

$$\odot\text{'s light} = 20.000.000.000 \times \text{the light of Sirius.}$$

V. Observations of the Light of the Sun, compared with that of a Candle, by means of Shadows.

Date of the Observation. 1799.	H Diameter of the Hole in the Shutter, in parts of an inch.	D Distance of the Hole from the Screen in Inches.	C Distance of the Candle from the Screen in inches when its light is equal to that of ☉, admitted through Hole.	Numerical value of the Expression $\left[\frac{12 \times D}{C \times H} \times 2 \tan ☉'s \frac{Diam.}{2}\right]^2$
End of May, and Beginning of June.	.0067	93	19.5	6152
	.0072	93	19.0	5611
	.0086	93	18.0	4382
	.0093	93	17.5	3965
	.0093	111.5	20.5	5228
	.0098	102	14.25	6477
	.0098	108	15.$\frac{1}{4}$	6410
	.0098	120	17.0	6299
May 28	.0098	120	17.5	5944
June 19	.0105	126	15.0	7770
	.0111	93	14.5	4054
	.0118	93	13.0	4463
				66755

$$\frac{66755}{12} = 5563$$

Hence 5563 is the number of Candles, which being placed at the distance of twelve inches, will give a light equal to that of the Sun.

VI. Observations of the light of the Moon compared with that of a Candle, by means of Shadows.

Date of the Observation.	Remarks.	Distance, in inches, of the Candle from a Screen, when its light is equal to that of ☾.
1799, May 16	☾'s Elongation 170°$\frac{1}{2}$	144
.... June 17	☾ Full	144

Hence, $☾ = \frac{1}{144} \times$ Candle placed at the distance of twelve feet,

and $☉ = 5563 \times \left(\frac{144}{12}\right)^2$ Moons.

$= 801.072$ Moons.

V. *On the water of the Mediterranean.* *By* WILLIAM HYDE WOLLASTON, *M.D. F.R.S.*

Read December 18, 1828.

THE object of the present communication is to do justice to the memory of my late friend, Dr. MARCET, by recording the result of one of his latest efforts in the cause of science.

In his examination of sea-water, of which he gave an account in the Philosophical Transactions for 1819, the specimens with which he had been supplied from different depths in the Mediterranean, had not been sufficient to show what becomes of the vast amount of salt brought into that sea by the constant current which sets eastward through the Straits of Gibraltar. For though the escape of the water of that current may be fully accounted for by its evaporation, which must be very rapid and copious on the sunny shallows of Africa, yet the salt which that water held in solution, must remain in the basin of the Mediterranean, or escape by some hitherto unexplained means of exit.

In the hope of obtaining a more abundant supply of water from the greatest accessible depths, especially near the Straits, he begged assistance from Captain WILLIAM HENRY SMITH, R.N. who was engaged to make a survey of certain parts of that sea, and supplied that officer with the apparatus for raising water from great depths, which was contrived by Mr. TENNANT, and is described in the communication already referred to.

The zeal with which Dr. MARCET himself prosecuted his inquiries was so well known, that others were always willing to second his efforts, from a confidence that their labour would not be unprofitably wasted; and Captain SMITH did not fail to take every opportunity of collecting specimens in the course of his survey. But when he heard that Dr. MARCET was no more, not being aware of the interest with which the specimens would be received and examined by many surviving friends, he was unfortunately but too ready to oblige

other persons with portions of his collection, which were afterwards applied by them to other objects.

Nevertheless, at the time when I had the good fortune to be introduced to Captain Smith, in the month of June 1827, he still retained in his possession three bottles, the remainder of his stock, and at my request most obligingly sent them to me for examination.

Happily, one of these is such as to accord in the most complete manner with the anticipation, that an accumulation of denser water might be found at great depths in the neighbourhood of the Straits, from which a counter-current beneath, though far less rapid, might carry westward into the Atlantic, as much salt as enters, with the eastward current near the surface, from that ocean into the Mediterranean.

The evidence of this will be comprised, indeed, in very few words: for though the two first specimens, taken at distances of about 680 and 450 miles from the Straits, and at depths of 450 and 400 fathoms respectively, do not exceed in density that of many ordinary samples of sea-water, yet the last, which was taken up at about 50 miles within the Straits, and from a depth of 670 fathoms, has a density exceeding that of distilled water by more than four times the usual excess, and accordingly leaves upon evaporation more than four times the usual quantity of saline residuum.

Hence it is clear, that an under-current outward of such denser water, if of equal breadth and depth with the current inward near the surface, would carry out as much salt below as is brought in above, although it moved with less than $\frac{1}{4}$th part of the velocity, and would thus prevent a perpetual increase of saltness in the Mediterranean Sea beyond that existing in the Atlantic.

On comparison of the relative specific gravities and quantities of salt, in the Table subjoined to this paper, with those in Dr. Marcet's Table, there may be remarked a want of accordance between the two experimenters, that will require to be explained.

This difference arises from the different temperatures at which his results and mine were dried. In his experiments the degree of heat chosen was 212°; in mine, the temperature was raised beyond 300°. In each case it will be seen that the quantity of saline contents to be obtained may be estimated from the specific gravity, by multiplying the excess of density above that of distilled

water by a certain factor, which will vary with the temperature that we may select for drying.

At 212° this factor is about .144, and the product will then represent the saline contents + a quantity of water retained by the deliquescent salts. At 300°, and upwards, the factor is only .134, on account of a nearer approach to perfect desiccation.

But as the leading question considered in this paper does not turn upon the estimate or actual weighing of any small differences in quantity, I have not thought it necessary to spend much time in endeavouring to obtain as precise a determination as resulted from the more careful manipulation of Dr. MARCET himself.

TABLE.

	Latitude.	Longitude.	Depth.	Sp. Gravity.	Salt per Cent.
	° ′	° ′			
No. 1	38 30	4 30 E.	450 fath.	1.0294	4.05
2	37 30	1 0 E.	400	1.0295	3.99
3	36 0	4 40 W.	670	1.1288	17.3
Gibraltar	36 7	5 22 W.			

VI. *An account of the preliminary experiments and ultimate construction of a refracting telescope of 7.8 inches aperture, with a fluid concave lens. In a letter addressed to* Davies Gilbert, *Esq. President of the Royal Society. By* Peter Barlow, *Esq. F.R.S. &c.*

Read December 18, 1828.

I HAVE great pleasure in forwarding to you the following account of the continuation of my experiments on the construction of refracting telescopes with fluid lenses; and after the interest you have taken in the experiments, and the recommendation you were pleased to give on the subject to the Board of Longitude, through whose aid I have been enabled to pursue them, I cannot but flatter myself that it will be satisfactory to you to submit this communication to the Royal Society, who have done me the honour of publishing my first proposition on this subject in their Transactions.

The instrument I intend more particularly to describe in this paper has a clear aperture of 7.8 inches, exceeding, I think, by about an inch the largest refracting telescope in this country. Its tube is 11 feet, which together with the eye-piece makes the whole length 12 feet; but its effective focus is, on the principle explained in my former paper*, 18 feet. It carries a power of 700 on the closest double stars in South's and Herschel's catalogue; and the stars are with that power round and defined, although the field is not then so bright as I could desire.

The telescope is mounted on a revolving stand, which works with considerable accuracy as an azimuth and altitude instrument, so as greatly to facilitate the direction of the instrument to any star whose right ascension and declination are given, although it may not be distinctly visible to the naked eye. To give steadiness to the stand it has been made substantial and heavy, its weight by estimation being 400 pounds, and that of the telescope 130 pounds; yet its

* Phil. Trans. 1828; Art. VII.

motions are so smooth, and the power so arranged, that it may be managed by one person with the greatest ease, the star being followed by a slight touch, scarcely exceeding that required for the keys of a piano-forte*.

In the first instance I erected this stand on a platform in my garden, but I soon found that exposure to the weather very much injured its action; moreover, the difficulty of mounting and dismounting the telescope was considerable, and liable to derange its adjustments. I was therefore almost under the necessity of erecting an observatory to contain it. This is an excellent light piece of carpentry by Mr. SMART of Lambeth, 16 feet clear in diameter, with a revolving conical roof rising 9 feet above the walls.

The roof contains 360 superficial feet, and weighs by estimation about 10 cwt. It is however by a simple apparatus made to revolve and open to any required azimuth, by the application of a force of about 10 or 12 pounds. The whole is well fitted up, and forms a neat light building, which by permission of His Lordship the Master General is erected on a piece of Ordnance ground adjoining my premises, commanding an entire view of the heavens for all altitudes exceeding 10°.

Having thus stated generally the nature of my operations, I shall proceed to explain them more particularly under distinct heads in the following pages.

Preliminary Experiments.

In my former paper (Phil. Trans. 1828: Art. VII.) I have endeavoured to show the effect which opening the lenses to different distances produces on the secondary spectrum; my first object, therefore, in these experiments was to ascertain by actual observation the best position of the lenses for the diminution of this defect.

In order the better to classify my experiments on this head, it will be best to refer to the original formula for the destruction of colour, given in my paper in the Phil. Trans. 1827: Art. XV. in which I have shown, that with open lenses we have, when the colour vanishes, $\frac{(f-d)^2}{ff'} = \delta$.

* I ought to state that I am indebted for the design, arrangement, and superintendence of the construction of this apparatus to Mr. JOHN KINGSTON, acting master millwright in His Majesty's Dock Yard at Woolwich: a highly ingenious and valuable member of that establishment.

Where f = focal length plate lens
f' = focal length fluid lens
δ = dispersive ratio
d = distance of the lenses

Or calling $f - d = nf$ = remaining focus of plate beyond the fluid, this becomes

$$\frac{n^2 f}{f'} = \delta \qquad (1)$$

$$\text{or } f' = \frac{n^2 f}{\delta} \qquad (2)$$

If now we call f'' the resulting focus from this combination, reckoning from the fluid, we have by common principles $\frac{1}{nf} - \frac{\delta}{n^2 f} = \frac{1}{f''}$

$$\text{Whence} \quad f'' = \frac{n^2 f}{n - \delta} = \text{resulting focus} \qquad (3)$$

$$\text{Consequently } f''' = \frac{nf}{n - \delta} = \text{equivalent focus} \qquad (4)$$

$$l = \frac{(n - 1 - n\delta)}{n - \delta} f = \text{whole length} \qquad (5)$$

From which equations all the relations between these six quantities, viz. f, f', f'', f''', n, and δ are readily determined; where it may be observed that f''' is the focal length of a telescope on the usual construction to which this telescope is equivalent, and l the whole length of the tube.

If we consider l, n, and δ as given quantities, we have

$$f = \frac{(n - \delta) l}{n - 1 - n\delta} = \text{plate focus} \qquad (6)$$

from which f', f'', and f''' may be determined.

It is obvious from this last equation, since n and l may be assumed at pleasure, (at least within all practicable limits,) that this form of telescope will admit of great variety of proportions between the different quantities, and that some classes of these have a practical advantage over others may be reasonably expected. From the experiments I have made, it appears to me that the secondary spectrum is reduced as the lenses are opened, or as n decreases, but that the general field is enlarged and improved by increasing the value of n.

I however directed my attention principally to the destruction of the secondary spectrum; and with this view I ordered two 4½-inch tubes, 5 feet long, to be fitted up to receive in succession lenses of different focal powers, depending principally upon the value given to n, which I assumed as follows: viz. $n = .60$, $n = .55$, $n = .50$, $n = .45$, $n = .40$, the length in each case being 60 inches. Resting on these numbers, the following values were determined, the plate glass having an index .515, the fluid .634, and the dispersive ratio .308.

Tabular value of the different quantities.

$n = .60$	$f = 39.72$	$f' = 46.42$,	$f'' = 48.97$	$f''' = 81.6$
$n = .55$	$f = 35.53$	$f' = 34.67$,	$f'' = 44.11$	$f''' = 80.2$
$n = .50$	$f = 33.30$	$f' = 27.02$,	$f'' = 43.35$	$f''' = 86.7$
$n = .45$	$f = 30.30$	$f' = 19.91$,	$f'' = 43.20$	$f''' = 96.0$
$n = .40$	$f = 25.62$	$f' = 13.30$,	$f'' = 44.56$	$f''' = 111.4$

I soon found, however, that it was impossible to get all the lenses of equally good material and figure; and as, in consequence, one defect might be mistaken for another, I altered my plan, and availed myself of the two telescopes I had constructed before, in one of which $n = .50$, and in the other $n = .54$. These two I had fitted with other lenses carefully made, making in one the value of $n = .60$, and in the other $n = .40$. I had also a new one made with the value of $n = .47$; and after a careful and patient examination of all these five, I determined, and I was supported in that determination by others, that the best effect was produced, at least as regarded the object I had in view, when the distance of the lenses was about one half the focal length of the plate lens, and with these proportions, therefore, I determined to construct my 8-inch telescope.

Construction of the Telescope.

Having as above stated decided that the distance of the lenses ought to be about half or a little more than half the focal length of the plate lens, I determined upon a focal length of 78 inches for my plate lens, and 59.8 inches for that of my fluid; which at the distance of 40 inches would produce a focal length of 104 inches, a total length of 12 feet, and an equivalent focus of 18

feet. For the curves of the parallel meniscus checks for containing the fluid, I proposed − 30 inches and + 144 inches, the latter towards the eye, and then computing the proper curves for the plate by the formula given in my paper, Phil. Trans. 1827: Art. XV. I found the proper curves to be 56.4 and 144, and to these curves Messrs W. and T. Gilbert worked the several glasses and the circular ring. Mr. Donkin undertook to draw the tubes, which I was desirous of having 8 inches in the interior diameter, but his nearest treblet was only 7.8 inches, to which size therefore I was confined. The tube was drawn in three pieces, each 3 feet 8 inches, making in all 11 feet; and to this the pipe for the eye-piece being attached, gave the full length 12 feet: two of the above pieces of 7.8-inch tube are strongly and accurately jointed by a lining piece, and the other part is made to screw on for more conveniently getting in and adjusting the fluid lens which is near this joint, and is inclosed in a cell which screws on to an interior tube 5 inches in diameter, and 3 feet 6 inches long, sliding in two collars properly turned for the purpose, having a notch in each to receive a feather attached externally to the tube to preserve a parallel motion.

The other end of this tube of course reaches to within about 4 feet of the eye end of the large tube, and to the former is fixed a brass nut properly fitted to receive a screw on the end of a brass rod 4½ feet in length; this rod works in a coupling box or collar, fixed on the inside of the large tube about 1 foot 9 inches from the end, and the end of the rod passes through the front end of the large tube, where it is cut square to receive a milled head or a universal joint key, by means of which the tube carrying the cell may be moved backwards or forwards; and the adjustment is thus made for colour in the first instance, and afterwards the focus is obtained by the usual rack motion.

The difficulty of centering two lenses at so great a distance from each other is considerable, if not properly provided for. In this instance the front lens is placed in a thin detached cell and confined by a counter cell. It is then placed with its first cell in another which screws and unscrews at the object end of the telescope as usual; except that the last cell is sufficiently large to admit of adjusting the interior one carrying the lens by means of two pair of opposite pushing screws. These provisions being made, the telescope is placed opposite to a proper object, the centering is produced by trial, by means of

these screws; and when every thing is right, the cell is made fast by four other screws, to prevent any trifling blow or other slight accident putting the glass again out of adjustment. In this state the telescope may be said to be completed; it has of course to be furnished with a finder, proper eyepieces, an apparatus for illuminating the field, &c., as in the usual cases.

With respect to inclosing the fluid, the following, after various trials, appears to me to be quite effectual. After the best position has been determined practically for the checks forming the fluid lens, these with the ring between them ground and polished accurately to the same curves, are applied together, and taken into an artificial high temperature, exceeding the greatest at which the telescope is ever expected to be used. After remaining here with the fluid some time, the space between the glasses is completely filled, immediately closed, cooled down by evaporation, and removed into a lower temperature: by this means a sudden condensation takes place, an external pressure is brought on the checks, and a bubble formed inside, which is of course filled with the vapour of the fluid; the excess of the atmospheric pressure beyond that of the vapour being afterwards always acting externally to preserve contact; the extreme edges are then sealed by the serum of human blood, or, which I believe to be equally efficacious, by strong fish glue and some thin pliable metal surface: by this process I have every reason to believe the lens becomes as durable as any lens of solid glass.

At all events I have the satisfaction of stating that my first 3-inch telescope has now been completed more than fifteen months, and that no change whatever has taken place in its performance, nor the least perceptible alteration either in the quantity or quality of the fluid. I must think, therefore, that the advantages to be gained by this means of supplying the flint glass are such as to entitle the experiments to an impartial examination; and I cannot doubt, if the prejudice against the use of fluids could be removed, that well directed practice would soon lead to the construction of the most perfect and powerful instruments on this principle, at a comparatively small expense. I am for instance convinced, judging from what has been paid for large object glasses, that my telescope, telescope stand, and the building for observation, with every other requisite convenience, have been constructed for a less sum than would be demanded for the object glass only, if one could be produced of the

same diameter, of plate and flint glass; and this surely is a consideration which ought to have some weight, and encourage a perseverance in the principle of construction.

The telescope and the particulars relative to it being thus described, it only remains for me to state the tests to which I have subjected it, and its performance in those cases.

The first observations of this kind are commonly on Polaris; the small star here is of course brilliant and distinct; it is seen best with a power of 120, but is visible with a power of 700.

The small star in Aldebaran is very distinct with a power of 120.

The small star in α Lyræ is distinctly visible with the same power.

The small star called by Mr. Herschel Debilissima, between 4 and 5 Lyræ, —whose existence, he says, could not even be suspected in either the 5 or 7-feet equatorial, and invisible also with the 7 and 10-feet reflectors of 6 and 9 inches aperture, but seen double with the 20-feet reflector,—is seen very satisfactorily double with this telescope.

η Persei, marked as double in South and Herschel's catalogue at the distance of 28″, with another small star at the distance of 3′ 57″ both n p, is seen distinctly sixfold, four of the small stars being within a considerably less distance than the remote one of η marked in the catalogue: and rejecting this remote star, the principal and the other four small stars form a miniature representation of Jupiter and his Satellites, three of them being nearly in a line on one side, and the other on the opposite: there are also other small stars within the same distance, but the most remarkable are those arranged in a line as above stated.

A number of other small stars which are spoken of as difficult to observe from their minuteness, are seen more or less distinctly with this instrument.

Amongst the closer and larger stars I have tried the telescope upon those commonly selected as tests, viz.

Castor; which is distinctly double with 120, and well opened and stars perfectly round with 360 and 700.

γ Leonis and α Piscium are seen, with the same powers, equally round and distinct.

In ε Bootis the small star is well separated from the larger, and its blue colour well marked with a power of 360.

η Coronæ Borealis is seen double with a power of 360 and 700; 52 Orionis, ζ Orionis, and others of the same class, are also well defined with the same powers.

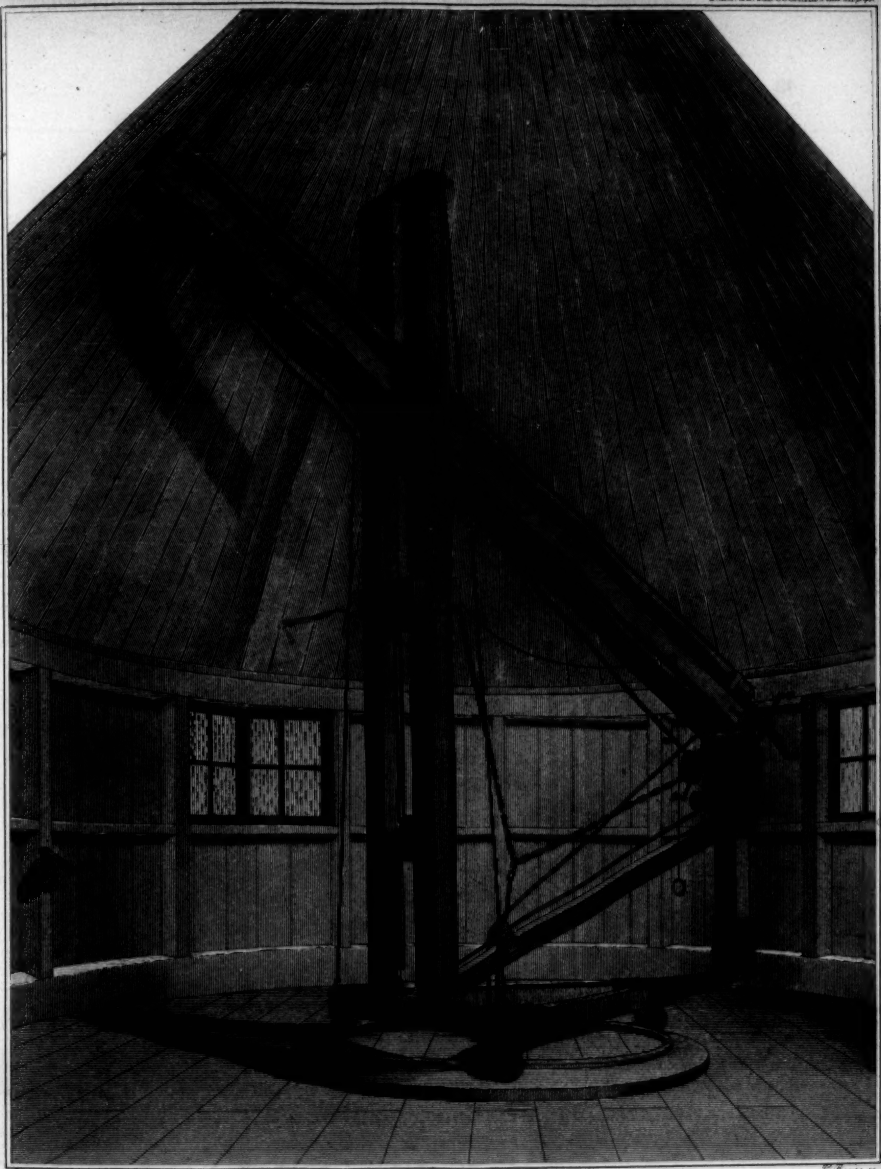
Still, however, it must be admitted that the telescope is not so competent to the opening of the close stars, as it is powerful in bringing to light the more minute luminous points.

Of the planets, I have only had an opportunity of trying the telescope on Venus, Saturn, and Mars; and the latter is too low to furnish a good test. Venus is beautifully white and well defined with a power of 120, but shows some colour with 360. Saturn with the 120 power is a very brilliant object, the double ring and belts being well and satisfactorily defined, and with the 360 power it is still very fine. The moon also is remarkably beautiful, the edges and the shadows being well marked, while the quantity of light is such as to bring to view every minute distinction of figure and shade.

Description of the Telescope Stand.

A correct representation of the stand, with the telescope upon it within the building, is given in Plate III. The fixed base is a strong built oaken curb six feet in diameter and six inches broad, imbedded on a circular brick foundation: to this is screwed an interior fillet or ribband, projecting higher than the other part of the curb, and serving the double purpose of keeping the stand to its centre and of receiving graduations to degrees and quarters, thus forming an azimuth circle. The outer part of the curb is beveled to the centre, and on this run three cast-iron rollers made to the same bevel: by these means the principal azimuth motion of the instrument is effected.

At the corners of the triangular base are three strong cast-iron sockets for receiving the ends of three oaken bars four inches square, which form the moveable base of the stand, the plate below each socket being a detached casting, screw-bolted through the timber and upper side, admitting thereby of being screwed up and tightened, in case any shrinkage takes place in the wood.



Drawn by W. H. Barlow.

W. Bawtree sc.

The front socket is different from the others, as it forms one piece with an upright socket which carries a strong oaken stancheon, to the upper part of which the machinery described below is attached. This stancheon having to support a great part of the weight of the telescope, at least in some positions of the latter, is strongly braced back by an oaken beam to the opposite ledge of the triangular base.

The principal upright part of the stand are two oaken bars framed or secured together in the middle and on the top, and turning on strong iron bolts in two cast-iron ears below; about which bolts this part, called the swinging frame, has a motion. These bars are cased on the outside by grain-cut oaken facings, and thus form two grooves in which an interior frame slides freely. This frame, on one of its interior sides carries a fixed racket, not seen in the plate, its whole length; and between the two connecting pieces in the centre of the swinging frame, is a spring pall which catches each tooth of the racket as it passes, being intended to prevent any accident in case of the breaking of a rope when the telescope is elevated.

To the pall is attached a string which descends near the upright stancheon; and when it is necessary to let down the telescope, this string is pulled by one hand, and the other having hold of the proper apparatus, the descending motion takes place in the most gentle and easy manner possible.

On the upper part of the front upright stancheon are two strong wrought-iron checks, terminating about two inches above it, having two circular holes $\frac{7}{8}$ths of an inch diameter, in which turns, as in two collars, a strong iron screw $1\frac{1}{4}$ inch in diameter, having two threads to the inch: on this works a strong brass nut with corresponding threads, and to this nut the frame which carries the telescope, and is called the bearing frame, is properly united, turning on a moveable joint near the screw. The screw is turned by four long cross handles, seen in the plate, by means of which the azimuth motion of a star or planet is followed. The length of the screw is about 11 inches and of the nut 3 inches, leaving a motion of 8 inches, which enables a star to be followed for a considerable time without moving the stand: the turning point on which this motion of the frame takes place, is exactly in the centre of the upper part of the interior or sliding frame, where a pin is fixed, which traverses in a parallel groove under the bearing frame; but to prevent confusion this is not shown in the plate.

On the upper part of the same interior frame are fixed two cast-iron rollers, on which the bearing frame rests, serving to relieve the machine of the friction that would otherwise take place when the telescope is raised or lowered. The two iron rods seen on each side, turning in two eyes below and adjustable at top by nuts and screws, were intended to serve as braces and to preserve steadiness; they are not, however, essential, as the instrument has every requisite stability without them.

Such is a general description of the stand; the manner of working it will be understood from what follows.

Below the fixed cross pieces, in the middle of the swinging frame, is a double fixed pulley, and to the lower part, on the inside of the sliding frame, is another double pulley, which rises and falls with the sliding frame. The end of the rope is fixed to the cross piece, descends and passes over one of the lower pulleys, thence over one of the upper, again descends, passes over the other lower pulley, then over the upper pulley, thence again to another single fixed pulley on the diagonal brace; it then passes over the lower barrel, which is turned by means of the wheel, pinion, and handle, shown in the plate. The power gained by the pulleys is 4 to 1, by the wheel and pinion 4 to 1, and by the barrel and handle 4 to 1, making in all 64 to 1. By these means the telescope may be raised even at its heaviest purchase with great facility.

This part of the machinery, however, is only intended to bring the telescope to an approximate altitude; after which, the part employed for bringing a star into the field, and for following it in altitude motion, is as follows.

At the extreme end of the bearing frame is a fixed pulley: to the back of the sliding frame, another; to the front of the same frame is another similar one; and a fourth at the other end of the bearing frame. The rope is first made fast at the extreme end, then passes over the pulley at the back of the sliding frame, thence over the pulley at the extreme end; whence it comes directly to the upper barrel; and after a few turns about this barrel, passes to the pulley on the front of the sliding frame, and returning passes over the pulley in the bearing frame near the upright stancheon, then over a fixed pulley on the brace: to the end of the rope is attached an iron weight of fifty-six pounds, which passes through a hole in the floor and hangs suspended in a well-hole below, serving thus to take in the slack of the rope, thereby keeping it always tight on the barrel, and also serving as a counterpoise to the swinging frame

when thrown out beyond its perpendicular position, as is necessary to bring the instrument to small angles of elevation.

This counterpoise is not, however, sufficient; another therefore is introduced, by suspending a chain from the swinging frame to the front stancheon; to the centre of which a 56-pound weight is suspended, passing over another pulley on the brace into the same well-hole: by this contrivance the tension of the chain increases as it approaches most to a straight line; that is, when the frame is thrown furthest out, and where its weight acts with greater force. By a slight adjustment of the length of the chain and weight, we may thus produce a perfect equilibrium in the whole machine, and the telescope is of course obedient to the slightest power sufficient to overcome the friction.

Things being thus equipoised, in order to render the motion as gentle as possible, a wheel and pinion are attached to the barrel last mentioned, similar to the one already described, but with four cross handles like those belonging to the screw. The power thus gained is 2 to 1 by the pulleys, 4 to 1 by the wheel and pinion, and about 9 to 1 by the handles, equivalent to 72 to 1. The slightest touch therefore of one of these handles will produce a change of elevation of the telescope, either to increase or diminish it, accordingly as that motion tends to pull in or out the swinging frame.

The operation therefore of putting the instrument on a star is: first, to swing round the whole stand towards the star, on the circular curb; then to bring it nearly to its proper altitude by the apparatus first described; then, being seated at a proper height, the eye being applied to the finder, with the handle belonging to the screw in one hand and that belonging to the altitude motion in the other, the star is brought immediately to the centre of the field, and is of course then in the large telescope. The observer is thus seated at perfect ease and follows the star at pleasure, one of the four handles on each side being always ready to receive a touch of the finger, which is sufficient for the purpose.

I have observed that this stand acts with considerable accuracy as an altitude and azimuth instrument: it may therefore be proper to say a few words on this subject. Such a purpose was not contemplated in its construction, and therefore, notwithstanding the usual accuracy of millwright workmanship, it could hardly have been expected to find the stand susceptible of such a degree

of accuracy; and it will not of course be understood that I am now speaking of extreme astronomical accuracy.

I found, however, the action so complete, that I determined to try how far it could be useful in this way. The lower curb was therefore carefully divided by hand into degrees and quarters, the meridian obtained by the best means in my power, an index fixed to the bottom of the frame and adjusted to the zero of the circle. A graduated circle, not seen in the plate, was then attached to the bearing frame with a suspended plummet, the telescope put upon a meridian star whose altitude was known, and the arc adjusted accordingly. With these apparently rough means, and another for converting right ascension and declination into azimuth and altitude; and with the help of an excellent pocket chronometer by Messrs. PARKINSON and FRODSHAM, my son, who has acquired great dexterity in the use of the instrument, can at any time select the right ascension and declination of the star from the catalogue, convert these into azimuth and altitude, direct the instrument towards the object, and be seated quite at his ease observing, in three minutes.

In a bright night,—and for observing a known star this is of course unnecessary; but for less conspicuous stars, which are scarcely distinguishable except by their catalogue positions, as also for finding any star before daylight is quite gone, or a planet in the day-time, these means, although far short of those afforded by an accurate equatorial, are very serviceable.

It should be observed that the stand was originally designed to work from the horizon to the zenith, which it is capable of doing; but I have limited its present action to an altitude of 65°, this being the greatest height I can obtain without cutting the upper curb of my observatory roof, which I am rather unwilling to do, for obtaining what is at best, with such an instrument, a very inconvenient position for observation.

Description of the Observatory.

It has been already stated that this room is circular, having a clear diameter of 16 feet. It is constructed as follows: A foundation wall 4 feet deep of 14-inch brickwork is first laid, and on this is imbedded a circular wooden curb in two thicknesses, each 2 inches deep and 4 broad, properly united with bolts, screws, and keys. Another exactly similar curb, united in the same way, forms the

upper part of the wall; and between these two are mortised 17 upright stancheons, each 2 inches by 4; and round the middle are framed other pieces of the same dimensions, viz. 2 inches by 4, cut also circular. These form the framing, which is lastly covered with inch boards properly tongued to keep out the wind and rain. Four windows are introduced, the size of the upper divisions, as seen in the plate, with a door not seen, being in the part supposed to be removed to show the instrument. The height of the boards is 6 feet 8 inches above the brickwork, and 7 feet 3 inches above the floor.

In another curb, exactly like those already described, are placed 12 iron rollers, which run on a circular plate of iron laid on the upper fixed curb. On the interior face of this moveable curb are also fixed 12 other iron rollers, which keep the curb to its centre by running against a plate of hoop iron, properly fixed to a fillet nailed to, but projecting above, the upper fixed curb.

This moveable curb is, as we have seen, 16 feet 8 inches exterior diameter, and forms the base of the roof. The latter is formed of about 60 six-inch boards cut nearly diagonally, the broader end being securely nailed to the bottom curb of the roof, and the smaller to an upper curb 2 feet 8 inches in diameter, the boards being each 12 feet in length and securely dowelled together. Two rafters, each 12 feet long, 2 inches broad, and 4 inches deep, placed parallel to each other from the upper to the lower curb, and $16\frac{1}{2}$ inches asunder, form an opening for observation; these are closed at other times by two shutters which turn on hinges in opposite ways the whole length of the roof. The joints of the boards of the roof are covered externally by canvass fillets of $1\frac{1}{2}$ inch in breadth, secured by white lead in oil; and lastly, the whole is protected by three thick coats of paint. The force necessary to overcome the friction of the roof is sixty pounds, and the motion is produced by an inch-and-a-half tarred rope passing externally round the moveable curb under the projecting eave-boards, which protect it from the weather: it then passes over two guide pulleys, and descends to a drum about 8 inches in diameter, turned to the proper curve for surging; it takes three turns round this drum, which is fixed to an axle that passes inside; this axle carries a toothed wheel, which is worked by a pinion and handle, seen in the plate. The power thus gained is 8 to 1; so that to move the roof ought to require but a force of eight pounds: but in consequence of the extra friction, it practically requires twelve pounds: it may, how-

ever, be turned round with comparative ease in about a minute. The two ends of the rope, as will have been understood, are fixed to the upper curb, crossing each other about 2 feet, viz. double the distance of the guide pulleys where they pass through holes in the curb to the inside, one of them being left so as to allow of taking in the slack of the rope, which is requisite in the beginning: but after being in use a few days this operation is no longer necessary, the rope being every where protected from the weather; viz. the part round the curb by the eaves of the roof, and the two descending parts by an external casing of wood. As the stand will work from very nearly a horizontal position to a vertical one, it was at first intended that, after the building was completed, the upper curb should be cut away, on one side, the breadth of the shutters, and its place supplied by an iron bolt; and thus, by having a shutter in the upper flat part of the roof, the instrument might have been brought vertical. This, however, has not been done; so that at present the limit of observation is between 10° and 65°; and through this range the instrument may be managed with the greatest possible facility by one person.

Such is the general description of my operations; and for the rest, I have only to express my hope, that this attempt to introduce a new principle of construction for achromatic telescopes will be examined with candour and impartiality: that the instrument is so complete and delicate in its action as the most perfect refractors which constitute the chefs-d'œuvre of opticians, will scarcely be expected. To produce such results requires a great deal of well directed practice, and selections from numerous attempts. I trust, however, I may say that the principle has been shown to be practicable, and that the result is by no means unsatisfactory: and when I state, that, with less than an ounce of the sulphuret of carbon, of the value of three shillings, I have supplied, in point of material, the place of the most perfect lens that could be procured of flint glass 8 inches in diameter, it will at least be admitted that the success of the experiment is not altogether uninteresting to the patrons and promoters of astronomical science.

I will only add, that I should feel no hesitation in undertaking the construction of another telescope of double the dimensions of the present.

VII. *On the Dip of the magnetic needle in London, in August* 1828. *By Captain* Edward Sabine *of the Royal Artillery, Secretary of the Royal Society.*

Read January 8, 1829.

THE Philosophical Transactions contain the record of the dip of the magnetic needle in London, observed at irregular intervals since the early part of the last century. In comparing these, and particularly the results obtained by Messrs. Whiston and Graham in 1720 and 1723, with those of Messrs. Nairne and Cavendish in 1772 and 1775, and both with the dip as it exists at present, we have satisfactory evidence of the progressive diminution of the dip in London during the whole of the period in question; but the observations are too few in number and infrequent, and the earliest ones particularly too doubtful in point of accuracy, to enable us to determine whether the annual diminution has been uniform or otherwise.

In the Philosophical Transactions for 1822, Art. I. the Society did me the honour to publish an account of observations which I had made in the Regent's Park, in August 1821, to obtain a correct determination of the dip in London at that time; in which observations I employed, for the first time in this country, a needle constructed on a plan proposed by Professor Meyer of Göttingen, for avoiding the usual error of dipping needles arising from the non-coincidence of the centres of motion and gravity. Seven years having since elapsed, an interval perhaps not too small to throw light upon the present rate of diminution of the dip, I repeated the observations in the August of last year, an account of which I now present to the Society; changing the place of observation, in consequence of the increase of buildings in the Regent's Park, to the Garden of the Horticultural Society at Chiswick, distant about six miles, in a direction coinciding as nearly as possible with the line of equal dip passing through the Regent's Park.

The general apparatus employed is the same that I used in the observations

of 1821: the limb is a circle of twelve inches diameter, each quadrant being graduated from 0° at the horizon to 90° at the vertical, and divided into spaces of 20′. The vertical edges of the agate supports, on which the axis of the needle rests, are rendered horizontal by a circular brass plate carrying a spirit-level; the lower surface of the plate is carefully ground, and being placed on the supports and turned successively in the four principal directions, the adjustment is made by the foot-screws of the instrument until the bubble of the level is stationary in every direction in which the plate is turned. The divisions 90°, and 90° of the circle marking its vertical points, ought then to coincide (and should they not do so, there is an adjustment to render them coindent,) with the points of conical radii, proceeding from the surfaces of the plate at right angles to it. By means of this plate, the level, by which the horizontal adjustment is effected, is applied directly to the supports, and the graduation of the circle made also to correspond with them.

The needle which was first used, was a flat needle of the ordinary construction 11.5 inches long, .4 broad, and .05 thick, rounded at the extremities. Three distinct observations were taken with it in each of the following positions; and as the arc was read at both ends of the needle, six readings were obtained in each position.—1st, with the face of the instrument to the east, and the marked side of the needle also to the east; 2nd, with the face to the east, and the marked side of the needle to the west; 3rd, with the face of the instrument to the west, and the marked side of the needle to the west; 4th, with the face to the west, and the marked side of the needle to the east. The poles were then changed, and the same course of observation gone through as before. The poles were changed by bar magnets in the usual way; the magnets were passed along the flat surfaces of the needle, ten times on each side; the needle being laid in a groove, which confined the motion of the magnets to a direction parallel to the needle. The force of magnetism imparted to the needle, on each occasion when its poles were changed, was measured by the time of performing 10 vibrations always in similar arcs. The horizontality of the supports was examined afresh every time the instrument was turned in azimuth; and no pains were spared to obtain results which might be consistent with each other, as I intended afterwards to apply the small screw and weight devised by Professor Meyer to the same needle, and wished to compare its perform-

ance as an ordinary needle, with its performance when used in the manner recommended by the Professor.

For the purpose of distinction, one of the ends of the needle had a mark upon it which the other end had not. In consequence of the axis of the needle not being perfectly coincident with the centre of gravity, the dip shown by it was too great when the marked end was a North Pole; and on the other hand, when the unmarked end was a North Pole, the dip shown was too small. After the two first experiments with the needle, this discrepancy was in great measure removed, by taking off a very small quantity from the marked end of the needle by means of a grind-stone. The following are the results obtained with this needle used in the manner described:

August 11th & 12th. From Noon to 4 P.M. Therm. 66°.

		Time of Vibs.			
Exp. I.	Marked end a S. Pole	53s.6	24 Readings	66° 23′.3	Mean 69° 26′.8
	Marked end a N. Pole	48 .4	24 Readings	72 30 .3	
Exp. II.	Marked end a S. Pole	52 .8	24 Readings	66 23 .1	Mean 69 37 .5
	Marked end a N. Pole	49 .6	24 Readings	72 51 .9	
Exp. III.	Marked end a S. Pole	46 .8	24 Readings	70 38 .8	Mean 70 14 .0
	Marked end a N. Pole	52 .8	24 Readings	69 49 .2	
				Mean . . .	69 46 .1

The same needle was then fitted with a small screw, inserted vertically in a line with the axis, and perpendicular to the needle; and brass beads of different sizes were successively used in the following experiments, in the manner and for the purposes recommended by Professor Meyer. Three distinct observations were made with the weight undermost when the face of the instrument was towards the east, and the same number with the face towards the west, both ends of the needle being read off on all occasions: the same process was then gone through with the weight uppermost; and finally the poles were changed, and the whole proceeding repeated. The dip is then deduced from the observations by the formula given for that purpose by Professor Meyer.

August 13th & 15th. Noon to 3 P.M. Therm. 67°.

		Time of Vibs.			
Exp. IV.	Marked end a S. Pole	53s.6	W. below;	61° 55′.5	Dip 69° 48′.3
			W. above;	80 30 .7	
	Marked end a N. Pole	54 .0	W. below;	58 38 .6	
			W. above;	80 25 .7	

August 16th. 3 to 5 P.M. Therm. 66°.

		Time of Vibs.			
Exp. V.	Marked end a N. Pole	54s.0	W. below;	53° 07′.0	Dip 69° 42′.0
			W. above;	87 11 .2	
	Marked end a S. Pole	53 .6	W. below;	58 55 .2	
			W. above;	84 37 .6	

August 20th. Noon to 2 P.M. Therm. 68°.

Exp. VI.	Marked end a S. Pole	53s.6	W. below;	57° 28′.7	Dip 69 58 .8
			W. above;	87 33 .4	
	Marked end a N. Pole	52 .8	W. below;	52 48 .3	
			W. above;	88 19 .8	

August 20th. 4 to 5 P.M. Therm. 67°.

Exp. VII.	Marked end a N. Pole	W. below;	52° 06′.0	Dip 69 40 .5
		W. above;	89 51 .0	
	Marked end a S. Pole	W. below;	57 12 .0	
		W. above;	86 13 .0	
			Mean . . .	69 47 .4

With the same general apparatus I employed also two other needles: one a flat needle with a cross of wires attached to the axis, two in the longitudinal direction of the needle, and two at right angles to it. By means of small weights sliding on these wires, the axis could be brought very nearly, if not exactly, to coincide with the centre of gravity. The same number of observations were made with this needle, and in the same general manner as in the experiments numbered I, II, and III. The following is the result obtained.

August 20th. 2 to 4 P.M. Therm. 69°.

Exp. VIII.	Marked end a N. Pole	24 Readings	69°34′.25	Dip 69°38′.3
	Marked end a S. Pole	24 Readings	69 42 .35	

The other needle was on a plan proposed some time ago by Mr. Dollond. The two ends of this needle are conical; the bases of the cones occupy two sides of a cube, which forms the middle of the needle; the other four sides being perforated for the purpose of receiving the axis in every direction. These sides are numbered for distinction 0, 1, 2, 3; 0 being opposite to 2, and 1 to 3. The axis is passed through the perforation until a shoulder on the one arm of the axis blocks against one of the sides of the cube, 0 for example: a nut is then screwed on the other arm of the axis against the side 2, until the axis is tightened in its place, care being taken that the same parts of the axis coincide

always with the longitudinal plane of the needle. The dip is then observed and registered with the face of the instrument successively towards the east and towards the west. The axis is then changed end for end, the nut that was before screwed against the side 2 being now screwed against the side 0; and the dip again observed with the face of the instrument east and west. The axis is then passed through the needle in a direction perpendicular to what it was before, the nut being screwed, for example, against the side 1, and the dip observed; the axis is then changed end for end, and the dip again observed. There are thus sixteen distinct observations in different positions of the axis and instrument: this number is doubled by reversing the poles and repeating the whole operation; and as both ends of the needle are required to be read, there are sixty-four arcs observed for each determination of the dip. In the present case two distinct observations were made in every position of the needle, axis, and instrument, and the dip is therefore an arithmetical mean of 128 observed arcs. In the following abstract the reference to the sides implies that the nut on the arm of the axis was screwed against that particular side of the cube in the observations referred to it.

August 13th & 15th. 3 to 5 P.M. Therm. 66°.

Exp. IX.	Marked end a N. Pole	Side 0; 70° 52′.0	70° 54′.9	Side 1; 70° 33′.8	70° 55′.3		
		Side 2; 70 57 .7		Side 3; 71 16 .8			
	Marked end a S. Pole	Side 0; 69 44 .5	68 48 .5	Side 1; 67 44 .0	68 48 .0		
		Side 2; 67 52 .6		Side 3; 69 52 .0			
			69 .51 .7		69 51 .65		
				Mean . .	69° 51′.67		

Captain FRANKLIN having kindly obtained from the Colonial Department the use of a small apparatus for determining the dip which he carried with him in his last land expedition, I am enabled to add the result of four series of observations made with it in the Garden at Chiswick, at the same time as the preceding observations, by Mr. DAVID DOUGLAS of the Horticultural Society. The circle is of six inches diameter only, being made so small for greater portability: except in size, the whole apparatus is in all respects similar to the one with which the observations already detailed were made. The agate supports are levelled in the same manner by a circular plate carrying a

level, and the mode of observation was the same in every particular. The needle was fitted with a screw and weights, on Professor MEYER's plan.

Exp. X. Aug. 18.	Marked end a N. Pole.	W. below;	52° 05′.5	Dip 69° 01′
		W. above;	89 38 .0	
	Marked end a S. Pole.	W. below;	56 11 .9	
		W. above;	85 12 .3	
Exp. XI. Aug. 19.	Marked end a S. Pole.	W. below;	62 24 .0	Dip 69 42
		W. above;	77 54 .0	
	Marked end a N. Pole.	W. below;	62 19 .0	
		W. above;	77 42 .5	
Exp. XII. Aug. 19.	Marked end a N. Pole.	W. below;	63 57 .5	Dip 70 38
		W. above;	77 46 .0	
	Marked end a S. Pole.	W. below;	63 42 .0	
		W. above;	78 22 .5	
Exp. XIII. Aug. 20.	Marked end a S. Pole.	W. below;	55 39 .0	Dip 70 04 .5
		W. above;	87 30 .0	
	Marked end a N. Pole.	W. below;	53 32 .0	
		W. above;	90 58 .0	
			Mean . . .	69 51 .4

The several results which have been enumerated, being collected in one view, are as follows:

Instrument of 12 inches diameter. Observer, Captain SABINE.

Exp. I, II, & III, with a common needle	69° 46′.1
Exp. IV, V, VI, & VII, with MEYER's needle	69 47 .4
Exp. VIII, with a needle having an adjustable axis . .	69 38 .3
Exp. IX, with Mr. DOLLOND's needle	69 51 .7

Instrument of 6 inches diameter. Observer, Mr. DOUGLAS.

Exp. X, XI, XII, & XIII, with MEYER's needle	69° 51′.4
Mean . . .	69 47 .0

Whence the final result of the dip in the Horticultural Garden at Chiswick, in August 1828, is 69° 47′ North.

The result of the observations in August 1821 was 70° 04′.5 (Phil. Trans. 1822: Art. I.); whence the dip in London appears to have diminished 17′.5 in

seven years, or 2′.5 in each year. This rate of diminution is sensibly less than the general average resulting from the comparison of the most authentic observations, at considerable intervals apart, in the century preceding 1821. These results fall variously between the limits of 2′.9 and 3′.2. Did the observations of 1821 and 1828 stand alone, in indicating a decrease at the present time of the amount of the annual change in the dip in this part of the world, it would appear the more probable supposition that either of those observations might be in error the few minutes which would be sufficient to make their difference correspond with former observations; and still more probable that they might contain between them an error of that small amount. But if we examine the very correct and consistent series of observations on the dip at Paris, commenced by M. Humboldt in 1798, and continued in subsequent years by MM. Gay Lussac, Humboldt, and Arago, we find in them a similar indication of diminution latterly in the annual decrease of the dip. If, for example, we divide the interval of thirty years between 1798 and 1828, into two nearly equal portions by means of the observations made by M. Arago in 1812, we have for the first portion, containing fourteen years, a diminution of ($69° 51′ - 68° 42′ = 69 \div 14 =$) 4′.93 a year; and for the second portion, containing sixteen years, of ($68° 42′ - 67° 58′ = 44 \div 16 =$) 2′.75 a year. And if instead of dividing the interval by M. Arago's observations in 1812, we take for that purpose the conjoint observations of MM. Humboldt and Arago in 1810, we have for the first portion, containing twelve years, ($69° 51′ - 68° 50′ = 61 \div 12 =$) 5′.08 a year; and for the last portion, containing eighteen years ($68° 50′ - 67° 58′ = 52′ \div 18 =$) 2′.89 a year: all which indications are of the same character and accord well with the observations of 1821 and 1828 in London.

A repetition of the observations in London at the expiration of another seven years, and a continuation of those at Paris, will probably show decisively whether the annual change in the amount of the dip in this part of the world is diminishing, as there now appears reason to suspect. Should it prove the case, careful and frequent observations of the dip will possess a more than ordinary interest, since the correct determination of the precise period when the dip may become stationary, and its amount at that time, which would be its minimum limit, will form most important additions to our knowledge of the phænomena of terrestrial magnetism.

VIII. *Remarks on the tendency to Calculous Diseases; with observations on the nature of urinary concretions, and an analysis of a large part of the collection belonging to the Norfolk and Norwich Hospital.* By JOHN YELLOLY, *M.D. F.R.S. &c.*

Read June 19, 1828.

HAVING, since my residence in the neighbourhood of Norwich, and my connection with the county hospital, paid considerable attention to calculous diseases and their concretions, I beg leave to lay some observations on those subjects before the Royal Society, to whose Transactions we owe much valuable information on the branches of pathology which relate to urinary complaints.

PART I.—*Of the tendency to Calculous Diseases.*

The county of Norfolk has long been remarkable for the occurrence of calculous diseases among its inhabitants; but there are no means of ascertaining how far this disposition extended, previous to the establishment of its hospital in 1772. Many of its cases went, of course, to the metropolis before that time; but there is, besides, every reason for concluding, that the operation of lithotomy was frequently performed in Norfolk, during all the preceding part of the eighteenth century, both from the reputation and extensive practice of Mr. GOOCH, one of the principal surgeons and surgical writers of his time, who lived near Norwich; and the occasional observations made by that gentleman in his surgical works, as to the skill, and experience in lithotomy, of practitioners in different parts of the county.

Operative surgery does not indeed, at this time, appear to have been confined to the regular surgeon; for in the little church of Stoke Holycross, about four miles from Norwich, is a mural monument of a clergyman, who died in 1719, and is represented, in an inscription surrounded by designs of various surgical

instruments, as having been distinguished for his abilities in theology, physic, surgery, and lithotomy*. .

From the establishment of the Norfolk and Norwich Hospital in 1772, to the end of last year, making a period of fifty-six years, 649 operations of lithotomy have been performed in it, which is at the rate of rather more than 11½ per annum, and about 1 in 40 on the total number of admissions, which amounted, in that period, to 26,521†. If we deduct from this number, the cases which have come from Suffolk and Cambridgeshire, amounting to 74, (of which, however, only a single instance has occurred from the latter county,) there will remain 575 furnished by the population of the county of Norfolk, which amounts to 351,000; and this will produce about 10.26 cases per annum, or 1 for every 34,000 inhabitants.

The number of cases arising in Norwich‡, in the same period, is 128, or about one fifth of the whole; while 447 are derived from the county of Norfolk, independently. Norwich, therefore, which contains 50,000 inhabitants, furnishes annually 2.28 cases on its population, or 1 for every 21,000 inhabitants; while the other parts of Norfolk afford only 7.98 per annum on their population of 301,000, or 1 for every 38,000 inhabitants, which is not much above one half of the proportion of Norwich.—Considerable differences likewise exist, with regard to the ratio of numbers furnished by the different hundreds of Norfolk; the eastern parts of the county, however, contributing more largely than the western. Thus the six western contiguous hundreds, including Lynn, have furnished not more than one half of the proportion of the six eastern hundreds, including Yarmouth; and the difference is still more striking with regard to some of the individual hundreds; for the hundreds of Taverham, Tunstead, and Walsham (contiguous hundreds on the eastern parts of the county), have regularly furnished about five times the proportion of the contiguous western hundreds of Freebridge Marshland, Freebridge Lynn, and

* Memoriæ Sacrum Thomæ Havers, Clerici, qui Theologiâ, Medicinâ, Chirurgiâ, et Lithotomiâ, doctus fuit et peritus &c.

† The hospital contains about 100 patients, and averages about 80.—I have adopted the census of 1820 in my calculations, and have usually put aside the hundreds.

‡ With Norwich I include, as is usual, what is called the County of the City of Norwich, a district which extends, in one direction, about two miles from the city, and in the others, from half a mile to a mile.

South Greenhoe; which proportions have been pretty much preserved, during every part of the period to which the records of the establishment extend.

It is to be observed, however, that there are some singular anomalies on this subject; for in a few instances it has happened, that a particular hundred has been remarkably free from the disease, and that the contiguous one has afforded rather an unusual number of examples of it.

There has been no material alteration in the number of cases which have occurred, in a similar space of time, during the different periods since the establishment of the Norfolk and Norwich Hospital; and hence, as the population of the county has augmented nearly a third during that period, the proportion of calculous cases may be considered as having diminished much in the same ratio.

With regard to the proportion in which calculous cases occur in other parts of the kingdom, the researches of Dr. Dobson*, and afterwards of Dr. Marcet†, and Mr. Smith of Bristol‡, have communicated the principal information which we possess upon the subject: but it is exceedingly difficult, from the want of efficient registers, to procure such data, as can connect the occurrence of a certain number of cases, with a certain known population. Mr. Smith's calculation, of the occurrence of about 47 operations for calculus annually in the hospitals of the metropolis, I believe to be pretty nearly the truth; and I have found from the registers of the London Hospital, (to which I was many years physician,) that in that establishment, about two thirds of the calculous cases have been furnished by the metropolis, and one third by the country. Taking the same proportions as applying to the other hospitals of the metropolis, I am therefore disposed to refer 31 of the 47 cases, to the population of the metropolis, amounting to rather more than 1,200,000 inhabitants; and 16 to about the same number living in counties adjacent to the metropolis, which possess no county hospitals, or have had them too recently established to affect

* Medical Commentaries, &c. with Observations on the Disposition to the Stone in the Cyder Countries, compared with some other Parts of England.

† Essay on the Chemical History and Medical Treatment of Calculous Disorders.

‡ A Statistical Inquiry into the Frequency of Stone in the Bladder in Great Britain and Ireland. Medico-Chirurgical Transactions, vol. xi. p. 7.

the calculation*. It would seem probable from this rough estimate, that 1 case of operation for calculus occurs annually for every 38,000 inhabitants in the metropolis, and about half that proportion in the counties contiguous to it.

From the information which Mr. SMITH has furnished, it appears that about 60 operations of lithotomy are performed annually in the provincial hospitals of England. This estimate includes Wales, whose sick poor, when sent from home, are chiefly transmitted to the hospitals of one or other of the adjoining English counties, as there are no such charities in the principality.—Suffolk did not possess a county hospital when Mr. SMITH was prosecuting his researches; and consequently was not included in his calculations, except as far as it furnished cases to the Norwich, or other hospitals. It possesses, however, in an eminent degree, the calculous character of Norfolk; and I have been enabled, through the kindness of some professional friends, to estimate the operations of lithotomy performed in it, by private practitioners, during the last 20 years, as about 4 annually. If this number be added to 1.26, (which is the annual proportion of 73 admissions from Suffolk to the Norwich Hospital in 56 years,) we shall have 5.26 cases as the annual product of Suffolk on its population of 234,000; or 1 case for every 44,000 inhabitants. It may be remarked, that the want of an hospital in Suffolk, till within these few years, and its distance, both from Norwich, and London, have occasioned the performance of a much larger proportion of operations of lithotomy in that county, by private practitioners, than is usual in other districts.

According to Mr. SMITH's calculation, there will therefore be 107 public operations in the whole of England and Wales, which, with the addition of 4 from Suffolk, will make 111 operations annually, on a population of very nearly 12,000,000, or 1 case for every 108,000 inhabitants. This, however, is not quite a third of the proportion which occurs in Norfolk.

If we put aside from the calculation, the 15½ cases occurring in the Norfolk district, (comprehending Norfolk and Suffolk) with its population, we shall then have 1 calculous case for every 118,000 inhabitants, independently of that

* Middlesex, Essex, Surrey, and Herts, may be regarded as hitherto principally dependent on the metropolis for Hospital accommodation; Kent, Sussex, Bucks, and Berks, as only partially so, perhaps to half their demand.

district. But if we further remove the cases which occur in London and the adjacent counties, with the respective population connected with them, we shall have not more than 49 examples of calculus attaching to the whole remaining population of England and Wales, or 1 case only, for every 188,000 inhabitants, which is little more than one fifth of the proportion of London, and of the Norfolk district excluding Norwich; and only about one ninth of the proportion of Norwich itself.

The tendency of any particular class of persons to be affected with calculous complaints, in the kingdom at large, must therefore be exceedingly small. But if we take individuals between 14 and 50, (which is the most extended period of active exertion in adult age,) the calculous cases will be still further reduced; for though it appears by the population returns, that nearly one half of the whole population of the kingdom, consists of persons between those ages, yet the calculous cases belonging to this period of life, as inferred from the Norwich register, are not quite a third of the total number. Under these considerations, I feel some degree of difficulty in completely assenting to the opinion which Mr. COPLAND HUTCHISON so ably supports*, of a sea-faring life being remarkable for the comparative infrequency of urinary calculi†.

In the Norwich as well as the London Hospital, the liability to calculous

* On the Comparative Infrequency of Urinary Calculi among Sea-faring People. Medico-Chirurgical Transactions, vol. ix. p. 443.

† By the register of the London Hospital it appears, that of 265 cases of calculus, which have occurred between the years 1761 and 1821, averaging 4½ per annum, 12 were of sea-faring persons, making 1 in 22 of this class of persons, in the whole admissions. Of these, 2 were under 14; 4 between 14 and 20; 3 between 20 and 40; and 3 above 40. Mr. BORRETT, an eminent surgeon of Yarmouth, some years ago operated on a lad of 12 years of age, who was on his fishing voyage at Yarmouth, from the North of England; and the same gentleman assisted Dr. TAIT, of the Naval Hospital there, about the year 1809, in an unsuccessful operation on a sailor, who was brought on shore, under great suffering, from a ship of war at that time lying in the roads. These are all the instances with which I am acquainted, of the occurrence of calculus in sea-faring persons; for the register of the Norfolk and Norwich Hospital does not afford any evidence on the subject, as it is only of late years, that the occupations of patients have been inserted in it. Without such an aid, I should feel it to be impossible to speak from recollection, as to the occupations of public patients, even if they had been known at the time of admission, which could be very seldom, and only incidentally the case.—The occurrence, according to Mr. HUTCHISON's interesting paper, of 6 cases of lithotomy in 15 years, in a naval population of 160,000, will be in the proportion of .4 per annum, or 1 in 400,000 persons. But if we put aside the London and Norfolk districts, as being more than ordinarily liable to the complaint; and comprise Scotland and Ireland in the calculation, which

diseases has been nearly as great, during the first sixteen years of life, as in the whole after period; but if we take the cases afforded by Norwich and London, independently of their respective country districts, as many cases of calculus have occurred below 14, as above that age; so that in those two instances, the proportion of children affected with this complaint, (judging from the hospital returns,) has been larger in a town, than in a country situation.

With regard to the mortality from the operation of lithotomy, the number of deaths in the Norfolk and Norwich Hospital, has been 89, which is a mortality of 1 in 7.29 cases, from the institution of the charity. But it is creditable to the state of modern surgery, and to the skill of the present surgeons of that Hospital, that in the operations performed by them (which amount to near one third of the whole number from the commencement), the proportion of deaths has been reduced to 1 in 8.42, which differs very little from the average of CHESELDEN, whose improved lateral operation they follow*.

afford their full complement of men to the British navy, but have less tendency to calculous diseases, according to our present evidence, than England; it will not be found, even if we take the smaller population of 1810, instead of that of 1820, that the proportion of those complaints, among large bodies of natives of the three kingdoms, between 14 and 50 years of age, acting together, will differ much from that stated by Mr. HUTCHISON.

* CHESELDEN is generally considered as having lost only 1 patient in 10½, in his hospital lithotomy practice; but the summary referred to, of 20 deaths in 213 cases, applied only to the success of his improved lateral operation. As far as existing documents afford evidence on the subject, (Anatomy, book iv. chap. 6; and Treatise on the High Operation, p. 17), that great surgeon, who was distinguished for candour and ingenuousness, lost, previously, 8 cases out of 28, which raised the average mortality of his hospital practice to 1 case in 8.6. His great success was in children; for the statement given by him, evinces his mortality to have been 1 in 5½, of even his most successful operations, in persons above 14. I may likewise remark, that it was foreign to CHESELDEN's plan, to notice any operations, but those immediately connected with his historical view of lithotomy; and he did not therefore record, in his publications, the number or result of any which occurred to him in St. Thomas's Hospital, between the time of his appointment to that charity in 1718, and his first mentioned high operation in 1722.—It is also important to bear in mind, in referring to CHESELDEN's mortality, that some of the patients who were included among his successful cases, were carried off by small-pox, in the Hospital, before their complete recovery. He gives it as his opinion, indeed, that such deaths were not in greater proportion, than might be expected to have occurred from small-pox alone; and the well-known uprightness of his character, entitles this opinion to every degree of credit. Hence, in strict correctness, they do not affect the accuracy of his general statement. In the Norfolk and Norwich Hospital, however, it is the invariable practice, for no case to be put down as a recovery from the stone operation, if the patient die in the hospital, of any disease which may even have come on during his convalescence.

Up to the age of 14, the deaths are only 1 in 14½; and above that age, 1 in 5¼. Between 14 and 40, the mortality is 1 in 10½; but after that period of life, it is augmented to the formidable extent of 1 in 3¾.

The number of female cases, in the whole, has been 31; the proportion, 1 in 20; and the deaths 1 in 15½. But from the improved practice by dilatation, all risk of life, in the abstraction of calculi from females, seems to be taken away. I have mentioned, in the Medico-Chirurgical Transactions, the removal from a female of a calculus of nearly 3½oz. troy, in weight, by spontaneous dilatation*; and some examples are given in the Philosophical Transactions, by Dr. Molyneux, Dr. Heberden, and others, of a similar kind: but it was not till of late years, that dilatation was employed to supersede the usual operation of lithotomy in women.

The following Tables will exhibit, at one view, the number of operations of lithotomy at the Norfolk and Norwich Hospital, with the mortality, at different periods of life.

Age or Sex.	Operations.	Cured.	Died.	Mortality.
Both sexes . . .	649	560	89	1 in 7.29
Males	618	531	87	1 — 7.1
Females	31	29	2	1 — 15.5
Both sexes.				
Under 14 . . .	292	272	20	1 — 14.6
14 and upwards	357	288	69	1 — 5.17
14 to 40	155	140	15	1 — 10.33
40 and upwards	202	148	54	1 — 3.74
14 to 50	196	171	25	1 — 7.84
50 and upwards	161	117	44	1 — 3.56
Under 16 . . .	317	294	23	1 — 13.78
16 and upwards	332	266	66	1 — 5.03

* Vol. vi. p. 574.

Age.	Operations.	Cured.	Died.	Mortality.
Inf. to 10	255	237	18	1 in 14.16
10 — 14	37	35	2	1 — 18.5
14 — 20	62	55	7	1 — 8.85
20 — 30	47	42	5	1 — 9.4
30 — 40	46	43	3	1 — 15.33
40 — 50	41	31	10	1 — 4.1
50 — 60	92	69	23	1 — 4
60 — 70	63	43	20	1 — 3.15
70 — 80	6	5	1	1 — 6

The operation of lithotomy is always attended with more danger, when calculi are large, than when they are small. This has been strikingly exemplified at Norwich; for of 52 cases of adult males, in which calculi of 2oz. or more occurred, 31 died, or nearly 2 in 3; while in 282 cases, also of adult males, in which the stones weighed less than 2oz., the mortality only amounted to 37, or rather less than 1 in 7. Part of this unfavourable issue, is no doubt to be attributed to the injury, both local and constitutional, which the long continuance of a large calculus in the bladder may occasion; but, at the same time, when we consider the general hazard of the operation of lithotomy, even in the most skilful and experienced hands, and the injury produced by the force necessary for extracting a calculus, and particularly a large one, there is a strong inducement afforded, to the full and dispassionate examination of the mechanical means which have been suggested, either for diminishing the magnitude of calculi, during an operation, in the revived and improved method of Mr. Henry Earle*, or for wearing them down, by slow and gradual detrition, according to the plan which has been employed at Paris by M. Civiale, so as in many cases to do away with the necessity of the operation altogether†.

The circumstances which occasion death after the operation of lithotomy, form an important and interesting subject of investigation to the surgeon; and

* Med. Chir. Trans. vol. xi. p. 69. † De la Lithotritie, ou Broiement de la Pierre dans la vessie.

I am inclined to think, that in addition to the unforeseen and unavoidable occurrences, which sometimes succeed the very best exertions of surgical skill, there is something to be attributed to the constitutional shock of a great operation, under which the system will occasionally sink. It is, however, a consideration, that may abate undue confidence from early success, on the one hand, and offer consolation and encouragement, under unexpected failures, on the other, that of two of the most distinguished contemporary lithotomists of this country, whose qualifications were of the most respectable kind, one lost 3 patients only of his first 50 operations, or 1 in $16\frac{2}{3}$; and 1 in $4\frac{3}{4}$ of the remainder, amounting to more than double, so as to reduce his average mortality to rather below 1 in 7; while the other lost 11 of his first 50 patients, or 1 in $4\frac{1}{2}$; and in his remaining cases, which a good deal exceeded those in the former instance, lost only 1 in $13\frac{1}{4}$, so as to render his whole average mortality rather less than 1 in 8*.

Recurrences of stone seem to be not very frequent; 14 instances, or 1 in 46 only, being found in the records of the Norfolk and Norwich Hospital, of the operation having been performed twice on the same individual: 3 were below 14, and 9, (of whom 2 died) above that age. The production of the second calculus took place, in 4 of these cases, within 1 year; in 5, within 2 years; in 3, within 3 years; while in the 2 remaining cases, the operation did not become again necessary, till after a lapse, respectively, of 7 and 10 years.

It does not appear that a second calculus is necessarily of the same character as the first. In the child from whom the first known specimen of cystic oxide was extracted, (that analysed by Dr. Wollaston), the disease returned; but a second operation was not submitted to. The child died, when a calculus, of a different character from the original one, was found in the bladder.

A curious example, of a similar kind, was shown to me at Cambridge, by Mr. John Okes, one of the surgeons to the Cambridge Hospital, of a calculus

* The examples occurred in the Norfolk and Norwich Hospital; and the gentlemen alluded to, were the late Dr. Rigby (formerly senior surgeon to that establishment), and the late Mr. Martineau.—I lament to say, that the death of the latter gentleman has taken place since my paper was laid before the Society.—The whole number of Dr. Rigby's hospital operations was 106, with 15 deaths; and of Mr. Martineau's 147, with 17 deaths. Mr. Martineau dated his principal success (Medico-Chir. Trans. vol. xi. p. 402.) from the time that he employed the blunt gorget, according to Cheselden's method; Dr. Rigby, as far as I have had an opportunity of ascertaining the point, always used the blunt gorget.

of cystic oxide having a lithic nucleus, which was removed from a boy of 4 years old, and was followed, in a short time, by the formation of another of fusible exterior, with a lithic interior, which made a second operation necessary in less than a year from the first. So speedily may the character of the animal process be changed, on which the formation or augmentation of urinary concretions depends.

With regard to the respective number of calculous cases which occur among the lower and higher orders of society, it is necessarily very difficult to obtain any correct information. Mr. Martineau, however, the senior surgeon of the Norwich Hospital, one of the most eminent and successful lithotomists of the present day, laid before the public, some years since, with much valuable information on the subject of lithotomy, a list of private patients, amounting to 10 in number, upon whom he operated, during a period that he operated on 111 public patients*. The proportion was 1 private patient to 11 public; which differs not much from the results of the late Mr. Brandon Trye of Glocester, as given in Mr. Smith's paper.

I regret that but little advances have been made, in a knowledge of the circumstances on which a tendency to calculous complaints depends; and I am not aware of such differences of air, water, soil, or habits of life having yet been detected, as can justify us in attributing the prevalence of stone, in the Norfolk district, to any of those causes.

A constitutional predisposition to the occurrence of calculous diseases, unquestionably exists in certain families. Dr. Prout, in his valuable work on Urinary Diseases, mentions an instance of a calculous tendency in three continuous generations; and I am acquainted with a family, where the grandfather, a man of active habits living in the country, was twice cut for the stone, and died from the second operation; the father was also cut; and two of the sons have exhibited an unequivocal calculous disposition, from an early period of life. I may also observe, that a few examples have occurred at the Norfolk and Norwich Hospital, where more than one individual of a family has had the disease, and undergone the operation.

The large employment of ill fermented farinaceous food, which marks in some degree the habits of the commonalty of Norfolk, may perhaps be regarded

* On Lithotomy. Medico-Chirurgical Transactions, vol. xi. p. 402.

as favouring the occurrence of calculous diseases; but a much coarser, and worse fermented material, in rye, barley, oats, and various mixtures of peas, with wheat or barley, has been, and perhaps still continues, to a certain degree, in use in Scotland, and the North of England, without being productive of such an effect. There are doubtless, however, various collateral circumstances that have not been sufficiently ascertained, which may have the power of modifying the effects of any particular description of food; and it is even very probable, that the laxative tendency of some of the coarser kinds of farinaceous aliment, may have a salutary influence, and obviate the disadvantages which might otherwise arise from their employment.

The cyder counties were at one time supposed to be peculiarly liable to calculous complaints; but so little ground is there for this opinion, that Herefordshire seems to have a very peculiar exemption from that malady; and Devonshire, not to have more than the average cases of other counties.

From the documents to which I have referred, it appears, that the tendency to produce calculus, is much greater in Norwich, and London, than in their respective country districts*. The same circumstance is very strikingly exemplified in Bristol; for according to Mr. SMITH's paper, to which I have had occasion so frequently to refer, 354 calculous cases have occurred in 82 years, at the Bristol Hospital, which is at the rate of 4.3 per annum. But of these, 173, or very nearly one half, were derived from Bristol and its liberties, which comprise a population of 87,000 persons; and 181 only, from the neighbouring districts, containing not less than 750,000 inhabitants†. The annual proportion would therefore be not less than 2.1 per annum for Bristol, which is 1 for 41,000 inhabitants; while in its extensive and populous country district,

* I have mentioned the calculous tendency of Norwich, as being nearly double that of the county at large. Four of the eastern hundreds, however, viz. Taverham, Walsham, Tunstead, and Happing, rather exceed Norwich in this disposition; having, in a joint population of 26,000, produced 75 cases of calculus, or 1.34 per annum. Those hundreds join each other; and one of them, Taverham, abuts on the Norwich district.

† Bristol, owing to its central position, receives patients from the counties of Somerset, Gloucester, Wilts, and Monmouth, as well as from South Wales; and I go upon the supposition (which I believe to be not far from the truth) that these districts transmit patients to the Bristol Hospital, in nearly an equal degree, with one or other of the hospitals of Bath, Exeter, Salisbury, Gloucester, or Hereford, according to local convenience.

the proportion would not be more than 2.2 per annum, or 1 only, for every 340,000 inhabitants.

In some parts of the country districts of Bristol, there are very singular anomalies; for the town of Chippenham, with only 3200 inhabitants, is stated by Mr. Smith, to have furnished as many cases of lithotomy, as the whole remaining county of Wilts. On the calculation of 18 cases in 82 years, or 1 in 4.5, which is about half the amount furnished by Wilts, there would, in this little town, be a tendency to calculous complaints, exceeding by about a fourth, that of Norwich itself.

Scotland is generally regarded as but little liable to the production of calculous diseases; and if Mr. Smith's calculation, of the occurrence of 8 cases only per annum, in that part of the island, is a correct one, it would, on its population of two millions, be in the ratio of 1 case for every 250,000 inhabitants. But the town of Dundee, in the county of Forfar, with a population of 30,000, has afforded to Mr. Crichton, of that place, in 36 years, 31 cases of stone, out of above 70 on which he operated during that period*. This is at the rate of .86 per annum, or 1 for every 35,000 inhabitants, if they had extended to that number. But if 5 are deducted, as having, from their designation in Mr. Crichton's list, the appearance of belonging to a higher class of society than enters into the calculations of this paper, we should then have .72 per annum, or 1 case for every 41,000 inhabitants, which is about the average proportion of Bristol.

In the instances which I have mentioned, it would therefore appear, that the tendency to produce calculous complaints, is greater in towns than in the country; and if this should prove to be the case generally, it would seem to indicate the existence, in children more particularly, of a connection between some diathesis which prevails in towns, (probably the scrophulous,) and the tendency to the secretion or deposition of lithic acid, on which the origin of urinary calculi so much depends. I have not had it in my power to ascertain, whether the greater disposition of towns to calculous complaints, applies more extensively than I have mentioned. I think it probable, however; but in some cases, in which I had expected to be able to connect the reports of the numbers operated upon, in a particular town or district, with a certain known

* Observations on the Operation of Lithotomy. Edin. Med. and Surg. Journal, vol. xxix. p. 225.

population, the records were not sufficiently ample to afford the requisite information. I did not therefore, avail myself of the well known kindness and courtesy of the medical officers of other provincial establishments, to trouble them with inquiries, which the plan of their registers might not, perhaps, give them the means of satisfying.

If I might venture, however, to make the suggestion, I would respectfully submit, how subservient our public hospitals, the boasts and ornaments of the country, might be made to important statistical inquiries, by a more extended system of registry, than is at present usually adopted, either in the metropolis, or in the country; and how conducive to pathological improvement, the information would be, which they might thus be so readily enabled to furnish.

The annexed Table will show the relation of calculous cases to population, in the principal instances which I have mentioned.

Place.	Population.	Number of Stone Cases.	Cases per Annum.	Comparative Frequency.
Norwich	56,000	128 in 56 yrs.	2.28	1 in 21,000
Norfolk, including Norwich	351,000	575———	10.26	1 — 34,000
Norfolk, excluding Norwich	301,000	447———	7.98	1 — 38,000
Suffolk	234,000	.....	5.26	1 — 44,000
Norfolk and Suffolk, including Norwich	585,000	.....	15.5	1 — 37,000
Norfolk and Suffolk, excluding Norwich	535,000	.....	13.33	1 — 41,000
London	1,200,000	.....	31.	1 — 38,000
Adjacent Counties	1,200,000	.....	16.	1 — 76,000
England and Wales	12,000,000	.....	111.	1 — 108,000
England and Wales, excluding Norfolk and Suffolk	11,415,000	.....	95.5	1 — 118,000
England and Wales, excluding Norfolk, Suffolk, and London, with its adjacent counties	9,015,000	.....	49.	1 — 188,000
Ditto persons between 14 and 50	4,134,000	.....	14.7	1 — 280,000
Bristol and its liberties	87,000	173— 82 yrs.	2.1	1 — 41,000
Bristol country district	750,000	181———	2.2	1 — 340,000
Scotland	2,000,000	.....	8.	1 — 250,000
Dundee	30,000	26— 36 yrs.	.86	1 — 41,300

Soon after the alkaline nature of Mrs. Stephen's remedies for stone in the bladder, was made known by her in 1739, in consequence of the recompense of 5000*l.* adjudged by Parliament for the disclosure, it was found by Dr. Hales, that caustic alkalies had the power of dissolving calculi out of the body. But Dr. Rutty*, and some years afterwards Dr. Dawson†, discovered that this substance was limited in its operation, to certain descriptions of calculi; while others were capable of being dissolved by nitric or muriatic acid alone; and hence they concluded, that in the latter cases, acids might be regarded as important lithontriptics. The subject, however, was not pursued; and these experiments, with the curative deductions made from them, were entirely lost sight of.

The precise nature of lithic acid was afterwards discovered by the celebrated Scheele; and for a long period subsequent to his time, urinary calculi were uniformly supposed to consist of this material, and alkalies, alone, employed in the treatment of the diseases which they occasioned. It is singular, however, that the calculi which formed the subjects of that great chemist's experiments, as well as of Bergman's, should have been so little varied, as not to have led to the observations which had been before made by Drs. Rutty and Dawson, and which would, in all probability, have opened the way to the further important discoveries in urinary concretions, which we owe, in so great a degree, to the perspicacity and talents of Dr. Wollaston.

In concluding the first part of the paper which I have the honour to lay before the Society, I would beg to observe, that I have put aside the consideration of private cases, except as regards Suffolk, because it is not likely that the proportion of these, will differ much, in different districts, from that of public ones.

I would also observe, that much valuable information with regard to the Norwich operations and collection, was brought forward by my able and excellent deceased friend Dr. Marcet, in his valuable work on Calculous Diseases, to which I have already alluded; but I have thought it desirable to present a general view of the whole subject, with such additional circumstances as I have had an opportunity of ascertaining.

* Account of some new Experiments and Observations on Joanna Stephen's Medicine for the Stone.

† On Human Calculi, showing them to be of different Kinds. Medical Transactions, published by the College of Physicians of London, vol. ii. p. 105.

Part II.—*Of Urinary Concretions.*

When I proposed an examination of the urinary calculi belonging to the Norfolk and Norwich Hospital, I had the expectation, that my attention would have been materially circumscribed by the previous labours of Dr. Marcet, who visited Norwich some years before, for the express purpose of examining the collection. I found, however, that none of the calculi contained in it were divided, and that the experiments instituted by our lamented colleague (of which an account was published in his work on Calculous Diseases,) were therefore necessarily confined to the outer surface, except in cases where the calculus had been broken in the extraction, and its interior structure thus allowed to be seen.

Within the last four or five years, a certain portion of the calculi have been divided; and these, as well as such as were broken in the extraction, amounting together to about 330, I have carefully analysed.

I wish I could have extended the examination over the whole collection, which consists of 649 specimens*; but as there is no very speedy prospect of the remainder being divided, so as to admit of a satisfactory analysis being made of them, I am unwilling, longer, to defer laying before the Society the results of my examinations, which exhibit a more extensive series of observations in this part of pathological chemistry, than has yet, as far as I know, been presented from any cabinet in this country. I shall be happy in embracing a future opportunity, of going through the whole remaining part of this splendid collection, should the division of the residue be effected within a convenient period. At the same time, however, it is not likely, that the proportions of the different descriptions of calculi which form the remainder, will differ materially from that of the large portion which I have analysed.—I have put the results of the analysis in a tabular form; and have stated, in the order of their occurrence from the centre, the consecutive deposits of the different materials of which the calculi are composed, according to the most prominent character of such material. I have said nothing of mixed calculi, or of calculi consisting, in the same apparent deposit, of mixtures of different ingredients; because it is

* This was the number up to the end of the year 1827. During the year 1828, there has been an addition of 11 specimens to the collection, from the occurrence of that number of operations.

well known, that no one deposit is strictly and unequivocally unmixed; but is blended, in various proportions, with others, so as to undergo, by this means, more or less modification of character and appearance.

Calculi consisting principally of one deposit.

Lithic acid	81
Lithate of ammonia	20
Oxalate of lime	20
Phosphate of lime	4
Fusible calculus, or mixed phosphates; that is to say, calculi composed of the triple, or ammoniaco-magnesian phosphate, united with phosphate of lime	37

Calculi consisting of two deposits.

Lithic acid and lithate of ammonia	37
——— oxalate of lime	11
——— mixed phosphates	10
——— phosphate of lime	2
Lithate of ammonia and lithic acid	2
——— oxalate of lime	25
——— mixed phosphates	14
——— phosphate of lime	1
Oxalate of lime and lithic acid	10
——— lithate of ammonia	1
——— mixed phosphates	15
——— phosphate of lime	3
Mixed phosphates and phosphate of lime	2

Calculi consisting of three deposits.

Lithic acid, phosphate of lime, and mixed phosphates	2
——— oxalate of lime, and phosphate of lime	1
——— oxalate of lime, and lithate of ammonia	2
——— oxalate of lime, and lithic acid	4
——— lithate of ammonia, and oxalate of lime	2

Lithic acid, oxalate of lime, and mixed phosphates	1
Lithate of ammonia, oxalate of lime, and mixed phosphates	3
oxalate of lime, and lithic acid	8
phosphate of lime, and lithate of ammonia	1
lithic acid, and mixed phosphates	2
Oxalate of lime, lithic acid, and lithate of ammonia	1
lithic acid, and oxalate of lime	1
lithic acid, and mixed phosphates	2

Calculi consisting of four or more deposits.

Lithate of ammonia, oxalate of lime, lithic acid, and mixed phosphates	1
Oxalate of lime, lithic acid, oxalate of lime, and mixed phosphates	1
Lithate of ammonia, oxalate of lime, phosphate of lime, oxalate of lime, and lithate of ammonia	1
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In this table, it will be seen, that about one half of the specimens are composed of one description of material only; and that the remainder consist of alternating layers, more or less numerous, of most of the substances of which human urinary calculi are composed.—On each of these substances, I shall make a few observations.

Of lithic acid and lithate of ammonia.

The distinction between these substances, though very generally recognized abroad, does not appear to have been much attended to in this country, till it was noticed by Dr. Prout, about nine years ago, in the Medico-Chirurgical Transactions*; and afterwards, by the same gentleman, in his important and interesting work on Calculous Disorders. The existence of lithate of ammonia, as a frequent component part of calculi, was distinctly pointed out, under the name of urate of ammonia, by Messrs. Fourcroy and Vauquelin, in their paper on Animal Concretions in the Annales des Chimie for 1799†; but as they very singularly omitted all notice of Dr. Wollaston's celebrated communication on a similar subject, which appeared in the Philosophical Trans-

* Vol. x. p. 389. † Tom. xxx. p. 57.

actions two years before*, it is not a matter of surprise, that the labours of these very eminent chemical philosophers, did not, in this department, obtain an authority in this country, which an appearance of greater candour would unquestionably have ensured them. Feeling, however, as I do, that Dr. Wollaston's paper, even after a lapse of above thirty years of the most active and successful period of chemical investigation, is not only to be regarded as a model of elegant and accurate deduction, but as containing nearly every thing of importance which is yet known on the subject of urinary calculi, I must still do Messrs. Fourcroy and Vauquelin the justice to state my conviction, that their operations were independent of those of our distinguished countryman. They were the first to notice lithate of ammonia; and their claim to originality may even derive some degree of support, from their having overlooked the most striking characteristic of the fusible calculus, noticed in Dr. Wollaston's communication,—ready fusibility, notwithstanding they were aware of the existence, as separate substances, of both the sets of materials of which it is composed, and knew also, that these substances are frequently united. The subject, it is also to be observed, was not a new one with them; for it appears from a paper which was published in the Annales de Chimie for the year 1793†, that M. Fourcroy had been engaged, at various periods since the year 1787, in prosecuting researches into the nature of animal concretions, during which he materially enlarged the bounds of our acquaintance with those substances.

Several of the specimens in the Norwich collection, bear a close resemblance to the plate which M. Fourcroy gives of a calculus of lithate of ammonia‡. They are small, gray, and laminated; and in addition to the usual characters of lithic acid, elicit ammonia copiously, on the addition of pure potash. Like those mentioned by Messrs. Fourcroy and Vauquelin, they are likewise generally derived from young subjects.

The combination of ammonia with lithic acid, is not, however, confined to small calculi, or to those which occur at an early period of life. It is to be found in calculi of all sizes, and belonging to all ages. But in such cases, ammonia invariably communicates a lighter colour, and diminishes the cohesion

* On Gouty and Urinary Concretions: Philosophical Transactions for 1797.

† Analyse Comparée des differentes Espèces des Concretions Animales et Vegetales, tom. xvi. p. 23.

‡ Annales du Museum d'Histoire Naturelle, tom. i. pl. vii.

of the calculus, as indicated on the addition of pure potash; though there is not, except occasionally towards the centre, the laminated structure of early life. In a few instances, the appearance of the lithate of ammonia calculus is not very dissimilar to the chalk-like excrement, or rather urine of the Boa Constrictor, which, as is well known, consists of lithate of ammonia*.

Immersion, for a few days, in pure ammonia, converts yellow laminated lithic acid, whether in small masses, or in powder, into light-coloured lithate of ammonia, from which ammonia is readily evolved by the action of pure potash, after that which is loosely adherent, has been carefully separated by distilled water. But the artificial addition of ammonia does not, as far as I have observed, communicate any degree of decrepitation to a lithic calculus, as might be imagined from an observation of Dr. Prout. It seems to be exceedingly likely, that some, at least, of the specimens of lithic calculus, which gave rise to Scheele's discovery of lithic acid in urinary calculi, really consisted of lithate of ammonia; since we are informed, that in his original experiments, a disengagement of ammonia took place, during the solution of the subjects of his analysis in liquid caustic potash, which would not have been the case, if the lithic acid, on which he operated, had been pure and uncombined†.

* Dr. Prout states to me, that he has never seen a calculus, essentially of lithate of ammonia, taken from a person after puberty; and is of opinion, that there are at least two varieties of the combination of lithic acid with ammonia, if not more.—It is exceedingly likely, that it is owing to the different quantities of ammonia, with which lithic acid, according to this very probable idea, is capable of being combined, with some diversity, perhaps, in the admixture of other substances, that the varieties observable in the appearance of lithate of ammonia, in calculi, are to be attributed.

I have had occasion, since this paper was laid before the Society, to examine some very minute round calculi, which were put into my hands by Mr. Dalrymple, now senior surgeon to the Norwich Hospital. They were 59 in number; were passed by a clergyman of about 54, all at once; and though they amounted only to three-eighths of a grain in weight, they occasioned considerable suffering prior to their discharge. They were amorphous in the centre, and laminated externally; and 35 were of very pure yellow lithic acid, the remaining 24, of gray lithate of ammonia, very similar in appearance to that which forms the lithate of ammonia calculus of children. They were formed, I have no doubt, in the kidney, and lay some time in its pelvis, before they were discharged. The circumstances of their occurrence resemble a good deal the cases mentioned in Dr. Prout's work (p. 135); but in this instance it would appear, that the production of lithic acid, and lithate of ammonia went on at, or nearly at the same time; and that adult age did not act as a bar to the formation of the latter. It is exceedingly likely, however, that the augmentation of any one of those minute calculi, if detained in the bladder as a nucleus, might not go on long with lithate of ammonia, except in early life.

† Chemical Essays of Charles William Scheele, translated from the Transactions of the Royal Academy of Sciences at Stockholm: Essay 9.

One of the most able and experienced of our English chemists, Professor Brande, has been induced to infer, that the evolution of ammonia from calculi, which were regarded as consisting of lithate of ammonia, depends, in all instances, on the decomposition of the ammoniacal salts contained in such calculi, and more especially of the ammoniaco-magnesian phosphate, with which the lithic acid is united. As this is an interesting point in the history of those substances, I have made it a particular object of attention; and the following is the result of my observations.

After exposing calculi bearing the character of lithate of ammonia, either to alcohol or distilled water, whether cold or heated, I have never found that by the abstraction of any substance from them, which those fluids were able to carry off, the development of ammonia was at all diminished, on subjecting the remainder to the action of pure potash. When a portion of such calculi, either before or after exposure to alcohol or distilled water, was submitted to the action of acetic acid, none of the crystals of triple phosphate were to be observed on the addition of carbonate of ammonia to the filtered liquor, though the existence of that salt is capable of being detected, by this process, in the most minute quantity. By adding pure potash to the dried deposit which is obtained by evaporation from a solution of lithate of ammonia in boiling water, a copious development of ammonia took place, and the deposit itself was capable of undergoing complete solution in pure potash. It does not therefore appear, that in those instances, the evolution of ammonia depended on the decomposition of triple phosphate.

Mr. Brande has shown, that muriate of ammonia is capable of being obtained from lithate of ammonia; and hence, he thinks, that this substance is one of those, which may afford the ammonia supposed to characterize the lithate of ammonia calculus*. My experiments coincide entirely with those of Mr. Brande, as to muriate of ammonia being a constant component part of that description of calculus; but besides that this substance is in too small a quantity to give rise to the elicitment of ammonia, which occurs on the addition of pure potash to a lithate of ammonia calculus, the disengagement of ammonia

* A Letter on the differences in the structure of calculi which arise from their being formed in different parts of the urinary passages, and on the effects that are produced upon them by the internal use of solvent medicines, from Mr. William Brande to Everard Home, Esq. F.R.S. Phil. Trans. for 1808. p. 231.

takes place, with equal freedom, after the muriate of ammonia has been withdrawn from it, as before. I would also add, that on heating dilute muriatic acid on the calculus, from which muriate of ammonia has been withdrawn by distilled water, muriate of ammonia is formed, by the union of such muriatic acid, with the ammonia which is in combination with the lithic acid in the calculus; an increase in the weight of the calculus, when thoroughly dried, is found to have taken place; and the newly formed muriate of ammonia, is freely given up to distilled water. It is not only cognizable by its mode of crystallization, but by its ready sublimation; by the extrication of ammonia from it by pure potash; by the deposition of triple phosphate on the addition of magnesia and phosphoric acid; and the formation of luna cornea, through the means of its muriatic acid, on the addition of nitrate of silver.

The existence of muriate of ammonia in lithate of ammonia calculi, led me to inquire, whether, as that substance is always found in urine, some portion of it may not insinuate itself into the other species of calculi, and not be an attendant on lithate of ammonia alone. On examining various urinary concretions with a view to this point, I have always found that muriate of ammonia is distinctly traceable in common lithic acid. It likewise exists in calculi of the mixed phosphates, and in those of oxalate and phosphate of lime; and is also capable of being detected in that very rare species of calculus, the cystic oxide*. The quantity, however, is exceedingly minute; but though perfectly sensible to appropriate re-agents, it is incapable of being detected by any development of ammonia which pure potash can render evident, either to the senses or to vegetable colours.

From these circumstances I am disposed to infer, that the ammonia by which so many of the lithic calculi are distinguished, which have come under my notice, is usually in actual combination with lithic acid, and does not arise

* The distilled water which takes up muriate of ammonia from calculi, withdraws likewise a certain portion of the lithic acid, or lithate of ammonia, from which hardly any calculus, except, perhaps, the cystic oxide, is altogether free. The peculiar crystallization in which the cystic oxide is disposed to be thrown down, in combination with the triple crystals, (when the latter are produced by the addition of magnesia and phosphoric acid to water, which has been boiled over that curious substance,) may render the existence of the triple crystals a little doubtful. On dissolving the latter, however, by acetic acid, which does not touch the cystic oxide, the triple crystals become apparent on the addition of carbonate of ammonia.

from the decomposition of an accessary ingredient, as in the specimens analysed by Mr. Brande.

Lithate of ammonia, whether natural or artificial, bears a temperature much above that of boiling water without decomposition; for it does not part with its ammonia when exposed to the heat of melted tin, which requires a temperature of about 440° for its liquefaction. Its ammonia, or at least the greater part of it, is, however, readily yielded to any dilute acid, particularly on the application of heat; and from the nature and amount of the saline compound which is formed, the quantity of ammonia existing in the lithate, might, I presume, be ascertained.

The lithic calculi form, as is usual, the most numerous class of concretions in the Norfolk collection, where they amount to nearly a third of the whole number analysed. But when, in addition, we take those into account, which have lithic acid, or lithate of ammonia as a nucleus, it appears that nearly three-fourths of such number (namely 238 out of 328) either consist of the lithates, or have those substances as their nuclei.

The same observation may be made as to about two-thirds, or 27 out of 41, of the calculi belonging to the Cambridge Hospital, of which the kindness of Dr. Haviland, the Regius Professor of Physic, and my other medical friends of that flourishing and well-regulated establishment, allowed me the particular inspection.

In the collection of calculi belonging to the University of Leyden, which I had an opportunity of examining about two years since, by the courtesy of Professor Sandiford, 38 out of 49 specimens which it contained, or three-fourths of the whole, bore the character which I have just mentioned.

Dr. Henry, of Manchester, published a valuable analysis in the Medico-Chirurgical Transactions some years since, of 187 calculi; and of those, 158, or five-sixths, were also either lithic calculi, or had lithic nuclei*. The evidence, therefore, which is derived from places far distant from each other, agrees as to the similarity in nature, of the primordia, of by far the larger proportion of urinary calculi; and evinces, that in appreciating the tendency to calculous disorders, and the means by which it is to be obviated, the attention

* Vol. x. p. 125.

must be particularly directed to the circumstances under which lithic acid is formed, or developed. The importance of this attention is put in a very striking point of view by Dr. PROUT, when he says, "that if a lithic acid nucleus had not been formed and detained in the bladder, two persons at least out of three who suffer from calculus, would never have been troubled with that affection."

A deposition of the phosphates, is not, according to Dr. PROUT's experience, followed by that of the other materials of calculi; and in this important particular, my observations, with hardly an exception, agree with his. Sometimes, indeed, I have seen little studs of lithic acid, or lithate of ammonia, imbedded in the mixed phosphates; but these appear to have descended from the kidney, as small calculi, and to have attached themselves to the phosphates during their existence in the bladder; for the laminated form which those phosphates often assume, is not interrupted under such circumstances, but only slightly altered in direction. Notwithstanding, however, the well marked character of the different species of urinary calculi, or their varied laminæ, there is still hardly a single deposit, with which a small portion of some of the other ingredients is not blended; a circumstance which probably arises, (as in the case of muriate of ammonia) from the readiness with which urine parts with a minute portion of most of its component parts.

Of oxalate of lime.

The calculus of oxalate of lime, has been generally designated as the mulberry calculus, from its resemblance, both in shape and colour, to a mulberry. Its appearance, however, varies from the darkest brown to a milk white, not differing much in colour from the fusible calculus. Its texture is generally tuberculated, or nodular; but this substance not unfrequently exists, in bright amber-coloured, or transparent white crystals, of the shape of flattened octohedrons. This form was noticed by M. FOURCROY, in concretions taken from the bladders of some animals, and was particularly observed by Dr. WOLLASTON, in three human calculi shown him by Dr. MARCET. There are not less than twenty examples, in the Norwich collection, of such crystallization: and I have seen a few examples elsewhere.—I had occasion to observe a similar form of crystal, in two or three small calculi of oxalate of lime, taken from the bladder

of a rat by a medical student some years since, and also in a calculus taken from the bladder of a pig. I have since found, that concretions of a similar description and form, are by no means uncommon in the former animal.

Of the triple phosphate, or ammoniaco-magnesian phosphate.

This substance is rarely found in its simple state, except as minute, transparent crystals, deposited between other laminæ. The nearest approach to it seems to be in the irregular white, or yellowish, or brownish-white crystallization, which is not unfrequently found on the surface of the mixed phosphates. This crystallization, I have always found to contain a small quantity of lime; and it must therefore, I presume, be considered, according to the division adopted by Dr. Wollaston, as belonging to the fusible calculi, or those consisting of the mixed phosphates, but possessing, perhaps, the smallest quantity of lime, which enters into the composition of this form of calculus. Dr. Prout is inclined to view it as having some unknown, but regular proportion of the two sets of ingredients. The varying proportions, of the two phosphates seem, indeed, (as Dr. Marcet very judiciously observes,) to communicate every degree of fusibility to the calculi which are composed of them.

The usual mode of detecting minute quantities of the triple phosphate, is, I believe, that recommended by Dr. Wollaston, of observing the formation of a white line, by the deposition of the crystals of that substance, on any part of the glass vessel containing them, which has been rubbed by a glass tube, or other pointed instrument. I have employed, however, what appears to me a still more ready mode of ascertaining the formation of the triple phosphate, by placing the fluid expected to contain it, in a watch-glass, in the field of a compound microscope of moderate power. The triple crystals are thus capable of being observed at the period of their earliest formation; and their gradual increase of size, and union with each other in various accidental ways, but mostly in a stellated form, come within immediate view, and form an interesting subject of observation.—It is the more desirable to have a ready and unequivocal mode, of determining the existence of minute quantities of the ammoniaco-magnesian phosphate, in the examination of animal bodies, as the production of that substance affords a means, to which it is difficult to find a limit, of ascertaining the existence of ammonia, magnesia, and phosphoric acid.

Of phosphate of lime, and the mixed phosphates.

During my analysis of the Norwich collection, I was accidentally led to suspect that carbonate of lime, though very unusual in urinary calculi in a separate and distinct form, was not an unfrequent concomitant of phosphate of lime. A particular examination of the collection, with a view to this special point, convinced me that such was the case. The existence of carbonate of lime was evinced by effervescence, on submitting a portion of the powdered calculus, to the action of dilute muriatic acid, in a small tube, after boiling it in distilled water, to extricate the atmospheric air involved in it. The gas evolved, was readily absorbed by pure potash over water; while pure ammonia deposited the phosphate of lime, leaving a portion of fluid, from which lime was thrown down by oxalate of ammonia.—The same circumstance, likewise, happened, when the muriatic solution was evaporated to dryness, and the dried portion submitted to distilled water; the muriate of lime, formed by the solution of the carbonate being dissolved, and the lime precipitated in the form of oxalate, by oxalate of ammonia. Carbonate of lime, I have likewise seen in the mixed phosphates, and so extensively, as to induce me to think it probable, that phosphate of lime is seldom or never found in urinary concretions, either separately, or in combination with the triple phosphate, uncombined with carbonate. This circumstance seems to be the less unlikely, when it is considered, that carbonic acid gas has been found to exist in a pure state in urine, and separable by the mere aid of diminished atmospheric pressure. This being the case, it may fairly be expected to unite with some portion of lime during the evolution of the latter, instead of suffering the whole of it to be employed in forming oxalate, or phosphate of lime.

I am happy in having had the kind assistance of Dr. Prout, and of Mr. Faraday of the Royal Institution, in ascertaining the existence of carbonate of lime, in some of the specimens of calculi in which that substance is not usually looked for. To Dr. Prout the circumstance was not unexpected; for he has long considered the existence of carbonate, with phosphate of lime in human concretions, exceedingly likely, though he had not put his ideas to the test of experiment.—An important confirmation of these observations I have likewise met with, in a paper by the distinguished Spanish chemist Proust, who states,

that in every instance of urinary concretion which came under his observation, he found carbonate of lime, when there was phosphate*.

In one or two calculi of mixed phosphates which are in Dr. PROUT's possession we found carbonate of lime; and the courtesy of Sir WILLIAM BLIZARD, the chairman of the Board of Curators of the Hunterian Museum, gave me the opportunity of making the same observation, in some of the specimens contained in that noble collection. I likewise had the particular favour from Dr. BENJAMIN BABINGTON, of not only examining with him, with the same result, several calculi of his small but valuable collection, (many of which are duplicates of those in the museum of Guy's Hospital,) but of being permitted the loan, and full use of his cabinet, which gave me the important opportunity, of instituting more ample experiments, than were at all admissible with the calculi belonging to the Norfolk and Norwich Hospital, where it was of course necessary to be limited to the smallest portion requisite for correct analysis.

I have likewise always found carbonate of lime in combination with phosphate, both in concretions formed in various parts of the body, and in prostatic calculi, (one of which I examined at the College of Surgeons,) although both sets of substances are generally regarded as consisting of phosphate of lime alone. The same observation has been made by Dr. PROUT, as to several similar substances which have come under his notice.

No specimen of cystic or xanthic oxide has yet been found in the Norwich collection.

The calculations which are comprised in this paper, can in many instances, only be regarded as approximating to the truth; and as depending on future, and more extended observation, for greater accuracy and precision. The inquiring disposition of the present age, has made us acquainted with the physical features of the principal parts of the united kingdom, whether in relation to power and variety of production, diversity of scenery, or peculiarities of geological character. A similar degree of talent and energy to that which has been so successfully employed on those objects, may be no less advantageously directed, to an examination of the habits and modes of life, by which the in-

* Annales de Chimie, tom. xxxvi. p. 263.

habitants of different districts are distinguished; to the description and influence of their food; to the maladies most prevalent among them; and the effect of their occupations, in producing, or modifying disease. These inquiries would lead to the knowledge of many important facts, which would not only be useful in throwing light on the nature of various obscure complaints, and in particular of those of a calculous description, but would have an extensive and advantageous bearing on general pathology.

I have alluded to a probable connection in towns, between the scrophulous, and the calculous diathesis; and in the Norfolk district, there is unquestionably a great disposition to every form of scrophula, from a slight enlargement of a submaxillary gland, to the severest states of articular, or pulmonary disease. Whether, however, the disposition is greater than in other counties; and how far, in this case, it may be connected with the calculous character of the district; future observations must determine.

Carrow Abbey near Norwich,
June 10, 1828.

IX. *Experiments to determine the difference in the number of vibrations made by an Invariable Pendulum in the Royal Observatory at Greenwich, and in the house in London in which Captain* KATER'S *experiments were made. By Captain* EDWARD SABINE *of the Royal Artillery, Secretary to the Royal Society. Communicated by the President and Council.*

Read December 11, 1828.

THESE experiments were made in compliance with a wish of the Council of the Royal Society, expressed in the following minute, dated December 13th 1827: "That Captain SABINE be requested to ascertain the difference in the number of vibrations of a pendulum between Mr. BROWNE'S house in London and the Royal Observatory at Greenwich."

The invariable pendulum employed to accomplish the proposed object was of the usual materials and form, new for the occasion, and numbered 12. The thermometer was the same that I had used in my former pendulum experiments; its graduation is described in the volume containing the account of those experiments, pages 182—187. The ball of the thermometer was suspended at both stations midway between the knife edge and the centre of the weight of the pendulum. The height of the barometer in the observations at Greenwich was taken by the standard barometer of the Observatory, which is in a room on the same floor as the pendulum room: in those at London it was taken by Mr. BROWNE'S barometer placed in the room in which the observations were made. Mr. BROWNE'S barometer being compared with the standard of the Greenwich observatory, by means of an intermediate portable barometer, was found to require a correction of + 0.066 to make it agree with the indications of the Greenwich standard corrected for capillary action. This correction is consequently applied.

The pendulum was first employed in experiments in Mr. BROWNE'S house from the 17th to the 20th of March inclusive: the rate of Mr. BROWNE'S clock

by CUMMING was furnished by himself. The observations are detailed in the Table A. at the close of the paper:—the following abstract exhibits the results:

London; March 1828; Experiments with Pendulum 12.

	Barom.	Therm.	Vibrations at 62°.
March 17.	30.132	63.22	85963.85
17.	30.129	63.45	85963.61
18.	30.125	62.82	85963.52
18.	30.040	63.175	85963.60
19.	29.480	62.32	85963.56
19.	29.485	62.42	85963.55
20.	29.375	61.15	85963.55
20.	29.270	61.32	85963.55
	29.975	62.5	85963.60

The height of the barometer corrected to the standard and reduced to 32° is 29.952.

The mean result is 85963.60 vibrations in a mean solar day, the pendulum being at 62°, the air at 62°.5, the barometer 29.952 inches, and the mercury 32°.

To reduce this result to the number of vibrations which would have been made had the pendulum vibrated in a vacuum, I have introduced for the first time a reduction obtained by direct experiment; namely, by vibrating the pendulum alternately in the air and in a rarefied medium very nearly approaching to a vacuum. The particulars of this experiment I hope shortly to communicate to the Royal Society; and may state in the mean time as its result, that the barometer being at 30 inches, the mercury at 32°, and the air at 45°, a pendulum, similar in form and materials to the one used on the present occasion, made 10.36 vibrations per diem less than when vibrating in a vacuum.

To adapt this reduction to the variations which the meteorological instruments undergo in different experiments, it will be remembered, that the specific gravity of air varies directly as the height of the barometer, and inversely as its expansion of $\frac{1}{480}$th part of its bulk for each degree of FAHRENHEIT.

The reduction in the present case, for barometer 29.952 inches, and thermometer 62°.5, is + 9.97; making 85973.57 vibrations at 62°.

In May the temperature in the room assigned by the Astronomer Royal for pendulum experiments, which is the western half of the quadrant room, having arrived nearly at the same height as during the experiments in London, a corresponding series was made at Greenwich on the 15th, 17th, 18th, and 19th of May, and 5th of June. The clock employed was one belonging to the observatory, made by GRAHAM, and was fixed against the south wall: its rate was supplied by Mr. THOMAS GLANVILLE TAYLOR, assistant at the Royal Observatory, by comparisons with the Greenwich transit clock, accomplished in the manner described in the memorandum B. annexed at the close of the paper. I am also indebted to Mr. TAYLOR for his zealous cooperation in these and the subsequent experiments at Greenwich: the observations made by him are distinguished by his name in Tables C. and F: the abstract of the results in Table C. is as follows:

Greenwich; May and June 1828; Experiments with Pendulum 12.

	Barom.	Therm.	Vibrations at 62°.
May 15.	29.765	60.07	85964.31
15.	29.755	61.15	85964.52
17.	29.705	59.70	85963.91
17.	29.715	60.68	85964.14
17.	29.700	61.32	85964.10
18.	29.695	59.28	85963.91
18.	29.715	60.05	85963.85
18.	29.705	59.77	85963.80
19.	29.710	57.85	85963.90
19.	29.710	58.70	85963.90
19.	29.700	59.00	85964.04
June 5.	29.335	54.92	85964.71
5.	29.360	55.75	85964.64
5.	29.385	56.65	85964.70
	29.639	58.92	85964.17

The height of the barometer, reduced to 32°, and corrected for capillary action, +0.019, (being the amount assigned by Mr. DANIELL for capillary depression in a boiled tube of 0.26 inch. diameter,) is 29.580.

The mean result is 85964.17 vibrations in a mean solar day, the pendulum

being at 62°, the air at 58°.92, the barometer at 29.580, and the mercury 32°. The reduction to a vacuum is + 9.92, making 85974.09 vibrations in a vacuum at 62°.

We have thus the vibrations of this pendulum at London and at Greenwich as follows.

London	85973.57
Greenwich	85974.09
Difference	0.52

Showing an acceleration at Greenwich of 0.52 parts of a vibration per diem.

Now as the latitude of the Royal Observatory is 2′ 28″ south of Mr. Browne's house in London, and as its height above the sea is also about 50 feet more, a retardation from these causes combined of about 0.3 of a vibration per diem was to have been expected at Greenwich, instead of an acceleration of 0.52 of a vibration. The result appeared therefore sufficiently remarkable to make it desirable to verify it by repetition.

The pendulum having remained at Greenwich a few days after the experiments were completed, the knife edge became slightly corroded with rust, in consequence of the great damp which prevails in the observatory at that season. The knife edge having been ground and figured afresh, the pendulum was again conveyed to Portland Place, and the experiments (Appendix D.) made with it on the 8th and 9th of July, the thermometer employed being the same as before, and suspended in a similar manner: the results were as follows.

London July 1828; Experiments with Pendulum 12.

		Barom.	Therm.	
July 8.	Mean of 7 exp.	29.670	72.07	85955.42 vibrations at 72°.
July 9.	Mean of 7 exp.	29.490	71.73	85955.40 vibrations at 72°.
		29.580	71.9	85955.41 vibrations at 72°.

The height of the barometer corrected to the standard and reduced to 32° is 29.533 inches.

The mean result being that the pendulum, having had its knife edge ground and figured afresh, made 85955.41 vibrations at 72°, equivalent to 85959.71

vibrations at 62°, the temperature of the air being 71°.9, the barometer 29.583, and the mercury 32°. The reduction to a vacuum is + 9.63, making 85969.34 vibrations at 62°.

The second series at Greenwich was commenced on the 21st of July, and continued to the 26th, both days included. The clock and thermometer employed were the same as on the former occasion; the rate of the clock was supplied by Mr. Thomas Glanville Taylor, by comparison with the transit clock of the observatory, as shown in the memorandum E. The experiments are given at length in the Table F, and their results collected in one view are as follows.

Greenwich, July 1828; Experiments with Pendulum 12.

	Barom.	Therm.	Vibrations at 62°.
July 21.	29.375	61.49	85960.16
22.	29.465	60.17	85959.70
23.	29.590	61.45	85959.89
24.	29.525	61.93	85959.82
25.	29.475	62.24	85960.04
26.	29.640	61.63	85960.15
	29.512	61.5	85959.96

The height of the barometer corrected for capillary action and reduced to 32° is 29.446.

The mean result is 85959.96 vibrations in a mean solar day at 62°; the temperature of the air being 61.5, the barometer 29.446, and the mercury 32°. The reduction to a vacuum is + 9.82, making 85969.78 vibrations at 62°.

We have thus a second series of the vibrations of Pendulum 12 at London and Greenwich, after its knife edge had been ground and figured afresh, as follows.

London.....	85969.34
Greenwich....	85969.78
Difference....	0.44

Showing an acceleration at Greenwich of 0.44 of a vibration per diem. The former result was an acceleration of 0.52 of a vibration per diem. We may therefore assume finally, 0.48 of a vibration per diem as the difference in

the rate of an invariable pendulum between the Royal Observatory and Mr. Browne's house in London; the pendulum vibrating quicker in Greenwich than in London.

The retardation computed for the difference in latitude between the two stations is 0.15 of a vibration per diem, and for their difference in elevation being about 50 feet and employing Dr. Young's co-efficient of .6, is 0.12 of a vibration per diem. The sum of the two computed retardations is 0.27; which added to the acceleration 0.48 shown by the experiments, makes altogether 0.75 of a vibration per diem; by which amount the result of experiment differs from what would have been anticipated, supposing that no previous experience had existed of the occurrence of such anomalies.

With regard to the fact, of the existence of this irregularity between Greenwich and London, it is one which admits of easy verification by persons who may be disposed to repeat the experiments: the stations are convenient and close at home; and the magnitude of the irregularity is such as to preclude uncertainty, since with proper precautions, it is not difficult to determine the relative rates of an invariable pendulum to nearly $\frac{1}{7}$th of the present irregularity.

With regard to its cause,—having already expressed the opinion that I had been led to form on the occurrence in my former pendulum experiments, of what I believe to have been irregularities of a similar nature,—it is unnecessary now to repeat that opinion; and having since seen no occasion to alter it, on the contrary much to confirm it, I gladly leave the discussion to others whose opinions are entitled to more weight.

Table A.

Vibrations of Pendulum 12 in London, March 1828.

Memorandum of the rate of Mr. Browne's clock by Cumming, from the 10th to the 24th of March: received from Mr. Browne.

March 10.	Cumming fast	4.05	
11.	————	3.95	losing 0.1 per diem.
13.	————	3.57	losing 0.19 per diem.
14.	————	3.64	gaining 0.07 per diem.
18.	————	3.63	keeping Mean Time.
19.	————	3.63	keeping Mean Time.
24.	————	4.01	gaining 0.07 per diem.

Exp. 1. March 17th 1828. Clock making 86400 Vibrations in a Mean Solar Day.

Barom. { begining 30.135 / ending 30.130 } 30.132

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.		Arc and Correction.		Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
		m s	m s	h m s	h m s	°	s	s	s	
62.9	1	50 55	50 58	10 50 56.5		1.16				
	2	57 29	57 33	10 57 31.0	10 57 30.5		+0.99	394.83	+0.52	85963.85
63.0	3	04 02	04 06	11 04 04.0						
63.5	25	28 48	28 55	1 28 51.5						
	26	35 24	35 29	1 35 26.5	1 35 26.5	0.45				
63.5	27	41 59	42 04	1 42 01.5						

Exp. 2. Fresh impulse given. Barom. { 30.130 / 30.128 } 30.129

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.		Arc and Correction.		Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
		m s	m s	h m s	h m s	°	s	s	s	
63.5	1	48 48	48 50	1 48 49.0		1.28				
	2	55 21	55 23	1 55 22.0	1 55 22.00		+1.19	394.36	+0.62	85963.61
63.5	3	01 54	01 56	2 01 55.0						
63.4	25	26 29	26 35	4 26 32.0						
	26	33 04	33 09	4 33 06.5	4 33 06.67	0.49				
63.4	27	39 38	39 45	4 39 41.5						

The reduction to a mean temperature of 62° is throughout computed in the proportion of 0.43 parts of a vibration per diem for each degree of Fahrenheit.

Exp. 3. March 18th. Clock making 86400 Vibrations. Barom. $\left\{\begin{matrix}30.15\\30.10\end{matrix}\right\}$ 30.125

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.		Arc and Correction.		Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
		m s	m s	h m s	h m s	°	s	s	s	
62.6	1	12 09	12 11	11 12 10.0		1.22				
	2	18 42	18 44	11 18 43.0	11 18 43.17		+1.17	394.51	+0.35	85963.52
62.8	3	25 15	25 18	11 25 16.5						
62.9	22	30 11	30 15	1 30 13.0						
	23	36 45	36 51	1 36 48.0	1 36 48.00	0.52				
63.0	24	43 22	43 24	1 43 23.0						

Exp. 4. Fresh impulse given. Barom. $\left\{\begin{matrix}30.08\\30.00\end{matrix}\right\}$ 30.04

63.2	1	13 05	13 07	2 13 06.0		1.04				
	2	19 38	19 40	2 19 39.0	2 19 39.17		+0.83	394.77	+0.49	85963.60
63.4	3	26 11	26 14	2 26 12.5						
63.0	23	37 45	37 53	4 37 49.0						
	24	44 21	44 28	4 44 24.5	4 44 24.17	0.43				
63.1	25	50 55	51 03	4 50 59.0						

Exp. 5. March 19th. Clock making 86400.07 Vibrations. Barom. $\left\{\begin{matrix}29.48\\29.48\end{matrix}\right\}$ 29.48

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.		Arc and Correction.		Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
		m s	m s	h m s	h m s	°	s	s	s	
62.1	1	04 05	04 07	11 04 06.0		1.05				
	2	10 37	10 41	11 10 39.0	11 10 39.33		+0.91	394.92	+0.14	85963.56
62.2	3	17 11	17 15	11 17 13.0						
62.5	21	15 40	15 45	1 15 42.5						
	22	22 16	22 21	1 22 18.5	1 22 17.83	0.48				
62.5	23	28 50	28 55	1 28 52.5						

Exp. 6. Fresh impulse given. Barom. $\left\{\begin{matrix}29.48\\29.49\end{matrix}\right\}$ 29.485

62.5	1	35 27	35 28	1 35 27.5		1.20				
	2	41 59	42 02	1 42 00.5	1 42 00.83		+0.94	394.84	+0.18	85963.55
62.6	3	48 33	48 36	1 48 34.5						
62.3	30	46 12	46 19	4 46 15.5						
	31	52 48	52 55	4 52 51.5	4 52 51.33	0.39				
62.3	32	59 22	59 32	4 59 27.0						

Exp. 7. March 20th. Clock making 86400.07 Vibrations. Barom. $\left\{\begin{matrix}29.42\\29.33\end{matrix}\right\}$ 29.375

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s	°	s	s	
60.7	1	39 59	40 02	9 40 00.5 } 9 46 34.83	1.11 } +0.87	395.42	−0.36	85963.55
....	2	46 34	46 36	9 46 35.0 }				
60.9	3	53 07	53 11	9 53 09.0 }				
61.5	27	31 17	31 24	12 31 20.5 } 12 37 55.83	0.40 }			
....	28	37 52	37 59	12 37 55.5 }				
61.5	29	44 28	44 35	12 44 31.5 }				

Exp. 8. Fresh impulse given. Barom. $\left\{\begin{matrix}29.33\\29.21\end{matrix}\right\}$ 29.27

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s	°	s	s	
61.5	1	50 55	50 56	12 50 55.5 } 12 57 29.12	1.12 } +0.75	395.46	−0.28	85963.55
....	2	57 29	57 30	12 57 29.5 }				
61.5	3	04 01	04 04	1 04 02.5 }				
61.1	34	28 19	28 27	4 28 23.0 } 4 34 59.33	0.32 }			
....	35	34 54	35 05	4 34 59.5 }				
61.2	36	41 30	41 41	4 41 35.5 }				

Table B.

Determination of the rate of the clock by Graham at the Royal Observatory Greenwich, from the 15th to the 20th of May inclusive, and from the 4th to the 6th of June inclusive: by Mr. Taylor.

"The comparison of the clock with the Greenwich transit clock was effected by means of a machine constructed by Hardy for the purpose, it being capable of indicating $0^{s}.05$ in time; and from the mean of 5 comparisons which was always employed, it is hoped the comparisons never err 0.03 from the truth; these comparisons were made at or near the time the observations were making for the rate of the transit clock, on the accurate determination of which must rest the accuracy of the rate of the clock used in the experiment.

For the rate of the transit clock the following observations have been selected from the Greenwich Observations.

1828.		Observed R. h m s	Apparent R. h m s	Error of Clock. s	Means.	Rate in 24 hours. s
May 15.	Castor...	7 24 8.57	7 23 37.76	+30.81	at 7h 29m +30s.72	
	Procyon..	7 30 49.34	7 30 18.68	30.66		
	Pollux...	7 35 18.54	7 34 47.85	30.69		
17.	Sirius ...	6 38 4.06	6 37 34.41	+29.65		
	Castor...	7 24 7.34	7 23 37.74	29.60	at 7h 17m +29s.57	−0.58
	Procyon..	7 30 48.06	7 30 18.66	29.40		
	Pollux...	7 35 17.44	7 34 47.83	29.61		
18.	Castor...	7 24 6.80	7 23 37.73	+29.07	at 7h 29m +29s.03	−0.53
	Procyon .	7 30 47.64	7 30 18.65	28.99		
	Pollux...	7 35 16.84	7 34 47.82	29.02		
19.	Castor...	7 24 6.44	7 23 37.72	+28.72	at 7h 29m +28s.75	−0.28
	Procyon .	7 30 47.33	7 30 18.64	28.69		
	Pollux...	7 35 16.64	7 34 47.81	28.83		
18.	γ Aquilæ	19 38 36.68	19 38 7.36	+29.32	at 19h 43m +29s.35	
	α Aquilæ	19 42 55.22	19 42 25.85	29.37		
	β Aquilæ	19 47 23.62	19 46 54.27	29.35		
19.	γ Aquilæ	19 38 35.96	19 38 7.38	+28 58	at 19h 43m +28s.73	−0.62
	α Aquilæ	19 42 54.52	19 42 25.88	28 64		
	β Aquilæ	19 47 22.98	19 46 54.30	28 68		

In addition to these, we have the following observations of the Sun.

	Observed R. h m s	Apparent R. h m s	Error of Clock. s	Rate in 24 hours. s
1828. May 14.	3 25 22.93	3 24 51.10	+31.83	
16.	3 33 15.85	3 32 45.20	30.65	−0.59
18.	3 41 11.00	3 40 41.50	29.50	−0.57
19.	3 45 9.76	3 44 40.40	29.36	−0.14
20.	3 49 8.59	3 48 39.90	38.69	−0.67

Collecting the results in the two preceding tables, we have,

May.	15 to 16	16 to 17	17 to 18	18 to 19	19 to 20	
	−0.58	−0.58	... ...	... ...	... ...	By Castor, Procyon, and Pollux.
	... ...	... ...	−0.53	... ...	... ...	By Sirius, Castor, Procyon, and Pollux.
	... ...	... ...	... ...	−0.28	... ...	By Castor, Procyon, and Pollux.
	... ...	... ...	... ...	−0.62	... ...	By γ, α, and β Aquilæ.
	−0.59	−0.57	−0.57	−0.14	−0.67	By the Sun.
Means	−0.58	−0.57	−0.55	−0.35	−0.67	

The ill accordance of the rates on the 18th—19th and 19th—20th, arises in a great measure from an indifferent observation of the sun on the 19th; and further it appears, that about this time the transit instrument was very unsteady, requiring adjustment both with regard to the meridian mark and level. These circumstances, combined with the steady going of the clock before and since this period, seem to justify the taking −0.54 as a mean rate for the whole time; which has been accordingly employed with the following comparisons in the determination of the rate of the clock used in the experiments.

1828.	Time by Clock. h m s	Time by Transit Clock. h m s	Sidereal Interval. h m s	Mean Interval. h m s	Clock rate in 24 hours. s
May 15.	23 15 0	4 18 35.85	24 23 52.85	24 19 53.58	+6.34
16.	23 35 0	4 42 28.70	25 0 29.08	24 56 23.82	+5.93
17.	0 31 30	5 42 57.78	25 17 1.73	25 12 53.77	+5.91
18.	1 44 30	6 59 59.51	24 14 51.69	24 10 53.89	+6.05
19.	1 55 30	7 14 51.20			
18.	19 34 0	0 52 20.31	12 45 1.79	12 42 56.75	+6.12
19.	8 17 0	13 37 22.10			

For the subsequent part of the experiments, the following transits have been selected from the Greenwich Observations.

1828.		Observed Æ. h m s	Apparent Æ. h m s	Error of Clock. s	Means.	Rate in 24 hours. s
June 4.	Regulus	9 59 35.40	9 59 13.74	+21.66		
	β Leonis	11 40 40.46	11 40 18.82	+21.64		
	Spica Virg.	13 16 33.10	13 16 11.29	+21.81		
	γ Urs. Maj.	13 41 8.92	13 40 47.35	+21.57	at 13h 22m	
	ε Bootis	14 37 52.47	14 37 31.08	+21.39	+21s.60	
	β Urs. Min.	14 51 42.00	14 51 20.42	+21.58		
	α Cor. Bor.	15 27 48.80	15 27 27.23	+21.57		
June 5.	Regulus	9 59 34.96	9 59 13.73	+21.23		
	α Urs. Maj.	10 53 24.47	10 53 3.33	+21.14	at 11h 53m	−0.42
	2 α Libræ	14 41 47.23	14 41 25.96	+21.27	+21s.21	
June 7.	Arcturus	14 8 11.80	14 7 51.68	+20.12		
	1 α Libræ	14 41 34.88	14 41 14.50	+20.38	at 14h 30m	−0.45
	2 α Libræ	14 41 46.30	14 41 25.96	+20.34	+20s.28	

Using these rates of the transit clock with the following comparisons of the experimental clock, we get its rate.

	Time by Clock. h m s	Time by Transit Clock. h m s	Sidereal Interval. h m s	Interval of Mean Time. h m s	Clock rate in 24 hours. s
June 4.	20 17 0	2 36 48.74	24 39 55.70	=24 35 53.68	+6.15
5.	20 53 0	3 16 44.44	26 11 10.21	=26 6 53.31	+6.16
6.	23 08 0	5 27 54.65			

THOMAS GLANVILLE TAYLOR."

Table C. Vibration of Pendulum 12 at Greenwich, May and June 1828.

Exp. 1. May 15th, 1828. Clock making 86406.34 Vibrations in a Mean Solar Day.
Barom. $\left\{\begin{matrix}29.77\\29.76\end{matrix}\right\}$ 29.765. Observer, Mr. T. G. Taylor.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s				
59.4	1	6 07	6 09	9 6 08.0				
59.6	2	12 37	12 39	9 12 38.0	9 12 38			
59.7	3	19 07	19 09	9 19 08.0	0.96	s	s	
60.4	21	16 21	16 34	11 16 27.5		+0.76	391.02	−0.83	85964.31
60.6	22	22 52	23 05	11 22 58.5	11 22 58.5			
60.7	23	29 23	29 36	11 29 29.5	0.45			

Exp. 2. Fresh impulse given. Barom. $\left\{\begin{matrix}29.76\\29.75\end{matrix}\right\}$ 29.755. Observer, Mr. T. G. Taylor.

°		m s	m s	h m s				
61.2	1	49 03	49 05	11 49 04.0				
61.3	2	55 33	55 36	11 55 34.5	11 55 34.33			
61.4	3	02 03	02 06	12 02 04.5	0.94	s	s	
60.8	21	59 14	59 25	1 59 19.5		+0.74	390.81	−0.36	85964.52
61.0	22	05 45	05 57	2 05 51.0	2 05 50.5			
61.2	23	12 15	12 27	2 12 21.0	0.44			

Exp. 3. May 17th. Clock making 86405.93 Vibrations. Barom. $\left\{\begin{matrix}29.70\\29.71\end{matrix}\right\}$ 29.705.
Observer, Mr. T. G. Taylor.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s				
59.2	1	29 42	29 43	18 29 42.5				
59.4	2	36 13	36 15	18 36 14.0	18 36 13.67			
59.5	3	42 43	42 46	18 42 44.5	0.94	s	s	
60.0	21	40 00	40 12	20 40 06.0		+0.74	391.173	−0.99	85963.91
60.0	22	46 31	46 43	20 46 37.0	20 46 37.17			
60.1	23	53 02	53 15	20 53 08.5	0.44			

Exp. 4. Fresh impulse given. Barom. $\left\{\begin{matrix}29.72\\29.71\end{matrix}\right\}$ 29.715.
Observers: 1—3 Mr. T. G. Taylor: 21—23 Captain Sabine.

°		m s	m s	h m s				
60.2	1	8 59	9 00	21 08 59.5				
60.3	2	15 29	15 31	21 15 30.0	21 15 30.17			
60.5	3	21 59	22 03	21 22 01.0	0.95	s	s	
61.0	21	19 14	19 24	23 19 19.0		+0.74	391.017	−0.56	85964.14
61.0	22	25 44	25 57	23 25 50.5	23 25 50.5			
61.1	23	32 16	32 28	23 32 22.0	0.43			

Exp. 5. Fresh impulse given. Clock making 86405.91 Vibrations. Barom. $\left\{\begin{matrix}29.70\\29.70\end{matrix}\right\}$ 29.70.
Observer, Captain Sabine.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s	°	s	s	
61.1	1	38 51	38 53	23 38 52.0				
61.2	2	45 21	45 24	23 45 22.5 } 23 45 22.33	0.92			
61.2	3	51 51	51 54	23 51 52.5		+0.68	390.79	−0.29 85964.10
61.4	21	49 01	49 14	01 49 07.5				
61.5	22	55 32	55 44	01 55 38.0 } 01 55 38.17	0.42			
61.5	23	02 03	02 15	02 02 09.0				

Exp. 6. May 18th. Clock making 86405.91 Vibrations. Barom. $\left\{\begin{matrix}29.69\\29.70\end{matrix}\right\}$ 29.695.
Observer, Mr. T. G. Taylor.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s	°	s	s	
59.0	1	55 28	55 29	18 55 28.5				
59.1	2	01 58	02 00	19 01 59.0 } 19 01 59.33	0.95			
59.3	3	08 30	08 31	19 08 30.5		+0.75	391.258	−1.16 85963.91
59.4	21	05 48	06 00	21 05 54.0				
59.4	22	12 18	12 31	21 12 24.5 } 21 12 24.5	0.44			
59.5	23	18 49	19 01	21 18 55.0				

Exp. 7. Fresh impulse given. Clock making 86406.05 Vibrations. Barom. $\left\{\begin{matrix}29.72\\29.71\end{matrix}\right\}$ 29.715.
Observer, Mr. T. G. Taylor.

°		m s	m s	h m s	°	s	s	
59.9	1	30 07	30 09	0 30 08.0				
60.1	2	36 37	36 39	0 36 38.0 } 0 36 38.5	0.95			
60.2	3	43 08	43 11	0 43 09.5		+0.75	390.90	−0.84 85963.85
59.9	21	40 19	40 31	2 40 25.0				
60.1	22	46 51	47 02	2 46 56.5 } 2 46 56.5	0.44			
60.1	23	53 22	53 34	2 53 28.0				

Exp. 8. Fresh impulse given. Barom. $\left\{\begin{matrix}29.71\\29.70\end{matrix}\right\}$ 29.705. Observer, Mr. T. G. Taylor.

°		m s	m s	h m s	°	s	s	
60.3	1	00 48	00 49	3 00 48.5				
60.3	2	07 18	07 20	3 07 19.0 } 3 07 18.83	0.92			
60.4	3	13 48	13 50	3 13 49.0		+0.68	391.033	−0.96 85963.80
59.2	21	11 02	11 14	5 11 08.0				
59.2	22	17 34	17 45	5 17 39.5 } 5 17 39.5	0.42			
59.2	23	24 05	24 17	5 24 11.0				

Exp. 9. May 19th. Clock making 86406.12 Vibrations. Barom. $\left\{\begin{matrix}29.70\\29.72\end{matrix}\right\}$ 29.71.
Observer, Mr. T. G. Taylor.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Corrections.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s		s	s	
57.2	1	14 49	14 51	19 14 50.0				
57.3	2	21 20	21 22	19 21 21.0	19 21 21.17			
57.4	3	27 51	27 54	19 27 52.5	0.94			
58.3	21	25 18	25 30	21 25 24.0		+0.74	391.708	−1.78	85963.90
58.4	22	31 50	32 01	21 31 55.5	21 31 55.33			
58.5	23	38 20	38 33	21 38 26.5	0.44			

Exp. 10. Fresh impulse given. Barom. $\left\{\begin{matrix}29.72\\29.70\end{matrix}\right\}$ 29.71. Observer, Mr. T. G. Taylor.

°		m s	m s	h m s		s	s	
58.5	1	58 01	58 03	21 58 02.0				
58.6	2	04 32	04 34	22 04 33.0	22 04 32.83			
58.8	3	11 02	11 05	22 11 03.5	0.96			
58.7	21	8 22	8 34	0 8 28.0		+0.76	391.35	−1.42	85963.90
58.8	22	14 55	15 06	0 15 00.5	0 14 59.83			
58.8	23	21 25	21 37	0 21 31.0	0.44			

Exp. 11. Fresh impulse given. Barom. $\left\{\begin{matrix}29.70\\29.70\end{matrix}\right\}$ 29.70.
Observer, Captain Sabine.

°		m s	m s	h m s		s	s	
59.0	1	53 40	53 41	0 53 40.5				
	2	00 10	00 12	1 00 11.0	1 00 10.83			
	3	06 40	06 42	1 06 41.0	0.97			
59.0	21	04 01	04 11	3 04 06.0		+0.77	391.35	−1.29	85964.04
	22	10 31	10 44	3 10 37.5	3 10 37.83			
	23	17 03	17 17	3 17 10.0	0.44			

Exp. 12. June 5th. Clock making 86406.16 Vibrations. Barom. $\left\{\begin{matrix}29.32\\29.35\end{matrix}\right\}$ 29.335.
Observer, Mr. T. G. Taylor.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc and Correction.	Mean Interval.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s		s	s	
54.2	1	08 54	08 56	21 08 55.0				
54.3	2	15 28	15 30	21 15 29.0	21 15 28.67			
54.5	3	22 01	22 03	21 22 02.0	0.95			
55.5	21	19 58	20 12	23 20 05.0		+0.75	393.5	−3.04	85964.71
55.5	22	26 32	26 46	23 26 39.0	23 26 38.67			
55.5	23	33 05	33 19	23 33 12.0	0.44			

Exp. 13. Fresh impulse given. Barom. {29.35 / 29.37} 29.36. Observer, Mr. T. G. Taylor.

Therm.	No. of Coincid.	Times of Disapp.	Times of Re-app.	Times of Coincidence.		Arc and Correction.		Mean Interval.	Reduct. to 68°.	Corrected Vibrations in a Mean Solar Day.
°		m s	m s	h m s	h m s	°	s	s	s	
55.5	1	39 25	39 26	23 39 25.5						
55.5	2	45 58	46 00	23 45 59.0	23 45 58.5	0.95	+0.74	393.17	−2.69	85964.64
55.5	3	52 30	52 32	23 52 31.0						
55.9	21	50 23	50 34	1 50 28.5						
55.9	22	56 56	57 08	1 57 02.0	1 57 02.0	0.43				
56.2	23	03 30	03 41	2 03 35.5						

Exp. 14. Fresh impulse given. Barom. {29.38 / 29.39} 29.385. Observer, Mr. T. G. Taylor.

°		m s	m s	h m s	h m s	°	s	s	s	
56.7	1	10 12	10 13	2 10 12.5						
56.9	2	16 43	16 45	2 16 44.0	2 16 44.5	0.94	+0.72	392.87	−2.30	85964.70
56.8	3	23 16	23 18	2 23 17.0						
56.4	21	21 03	21 14	4 21 08.5						
56.5	22	27 36	27 47	4 27 41.5	4 27 41.83	0.43				
56.6	23	34 10	34 21	4 34 15.5						

Table D.—London, July 1828. Experiments with Pendulum 12.

July 8th. Clock making 86400 Vibrations in a Mean Solar Day. Barom. {29.68 / 29.66} 29.67

No. of Coincid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc of Vibration.	Mean Interval.	Correction for Arc.	Reduct. to 72°.	Corrected Vibrations in a Mean Solar Day at 72°.
	°	m s	m s	h m s	°	s	s	s	
1	71.7	43 29	43 33	9 43 31.0	1.115				
6	71.7	15 44	15 49	10 15 47.5	0.915	387.45	+1.44	−0.13	85955.31
11	71.7	48 02	48 09	10 48 05.5	0.775				
16	71.7	20 23	20 30	11 20 26.5	0.625	388.25	+0.68	−0.11	85955.49
21	71.8	52 43	52 53	11 52 48.0	0.520				
26	71.9	25 04	25 16	12 25 10.0	0.450	388.40	+0.33	−0.04	85955.39
31	72.0	57 26	57 38	12 57 32.0	0.380				
36	72.1	29 46	30 03	1 29 54.5	0.310	388.45	+0.18	+0.04	85955.38
41	72.2	02 07	02 26	2 02 16.5	0.260				
46	72.3	34 29	34 50	2 34 39.5	0.210	388.55	+0.08	+0.13	85955.49
51	72.4	06 50	07 14	3 07 02.0	0.190				
56	72.4	39 10	39 40	3 39 25.0	0.160	388.50	+0.04	+0.17	85955.43
61	72.4	11 30	12 04	4 12 47.0	0.120				
66	72.4	43 50	44 30	4 44 10.0	0.100	388.55	+0.02	+0.17	85955.47
71	72.4	16 09	16 56	5 16 32.5	0.090				
	72.07								85955.42

July 9th. Clock making 86400 Vibrations. Barom. $\left\{\begin{matrix}29.50\\29.48\end{matrix}\right\}$ 29.49

No. of Coll. set	Therm.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc of Vibration.	Mean Interval.	Correction for Arc.	Reduct. to 75°.	Corrected Vibrations in a Mean Solar Day at 75°.
	°	m s	m s	h m s	°	s	s	s	
1	71.5	33 45	33 48	9 33 46.5	1.40	386.90	+ 2.23	−0.17	85955.44
11	71.7	38 13	38 18	10 38 15.5	0.95	387.85	+ 1.00	−0.13	85955.33
21	71.7	42 50	42 58	11 42 54.0	0.63	388.40	+ 0.47	−0.15	85955.42
31	71.6	47 32	47 44	12 47 38.0	0.45	388.55	+ 0.23	−0.17	85955.34
41	71.6	52 16	52 31	1 52 23.5	0.31	388.65	+ 0.11	−0.15	85955.34
51	71.7	56 58	57 22	2 57 10.0	0.21	388.80	+ 0.05	−0.15	85955.46
61	71.6	01 43	02 13	4 01 58.0	0.16	388.85	+ 0.03	−0.15	85955.50
71	71.7	06 25	07 08	5 06 46.5	0.11				
	71.73								85955.40 at 75°

Table E.

Determination of the rate of the clock by Graham, with which the pendulum was compared in the experiments at Greenwich between the 21st and 26th of July inclusive: by Mr. Taylor.

"The rate of the clock was obtained from comparisons with the transit clock at the time that the observations were making for the determination of the rate of the latter; thus resting the accuracy of the results on the correct determination of the rate of the transit clock, for which purpose the following observations have been selected from the Greenwich observations:

1828.		Observed Place. h m s	Apparent Place. h m s	Error of Clock. s	Mean.	Rate in 24h.
July 21 A.M.	Capella . .	5 4 55.92	5 4 1.69	+54.23	at 5h 23m + 54″.20	
	β Tauri. . . .	5 16 21.40	5 15 27.22	54.18		
	α Orionis . .	5 46 47.24	5 45 53.04	54.20		
—— 22 A.M.	β Tauri. . . .	5 16 20.98	5 15 27.25	53.73	at 5h 16m + 53″.73	−0″.47
—— 23 A.M.	Aldebaran	4 26 58.62	4 26 5.41	53.21	at 5h 42m + 53″.17	−0″.55
	Capella . .	5 4 54.96	5 4 1.75	53.21		
	Pollux . . .	7 35 41.04	7 34 47.96	53.08		
—— 25 A.M.	Aldebaran	4 26 57.56	4 26 5.46	52.10	at 4h 59m + 52″.07	−0″.56
	Capella . .	5 4 53.90	5 4 1.82	52.08		
	Rigel. . . .	5 7 9.80	5 6 17.74	52.06		
	β Tauri. . . .	5 16 19.38	5 15 27.33	52.05		
—— 26 P.M.	α Cor. Bor.	15 28 18.10	15 27 26.79	51.31	at 15h 32m + 51″.29	−0″.55
	α Serpentis	15 36 42.10	15 35 50.83	+51.27		

In addition to the preceding, two observations only of the Sun are available, which occurring at a time when only one star could be taken, are better employed in conjunction with that star; thereby more nearly equalizing the share of credit to be attributed to the several results.

		h m s	h m s	″	
July 21	⊙ Centre	8 3 50.05	8 2 55.50	54.55	− 0″.43
22	⊙ Centre	8 7 48.82	8 6 54.70	54.12	

Giving this result the same weight as that of β Tauri on the 22nd, where it is probable the greatest error exists, we have for the daily rate of the transit clock as follows.

	″	m s	
July 21 to 22.	−0.45	or +3 55.46	on mean time.
23.	−0.57	+3 55.34	
24.	−0.56	+3 55.35	
25.	−0.56	+3 55.35	
26.	−0.55	+3 55.36	

Making use of these rates with the following comparisons, the daily rates of the coincidence clock can be obtained up to the 26th; and since no observations offer to determine the rate on the 27th, the rate as determined up to within twelve hours of the comparison on the 27th (−0″.55) may with safety be used.

1828.	Time by Clock. h m s	Time by Transit Clock. h m s	Interval by Transit Clock. h m s	Interval of Mean Time. h m s	Interval by Clock. h m	Rate of Clock in 24 Hours. ″
July 21.	7 46 0	3 41 0.39				
			24 42 0.41	= 24 37 58.08	24 38	+ 1.87
22.	8 24 0	4 23 0.80				
			23 37 50.10	= 23 33 58.38	23 34	+ 1.65
23.	7 58 0	4 0 50.90				
			24 46 1.08	= 24 41 58.21	24 42	+ 1.74
24.	8 40 0	4 46 51.98				
			23 45 51.52	= 23 41 58.48	23 42	+ 1.54
25.	8 22 0	4 32 43.50				
			24 2 54.45	= 23 58 58.61	23 59	+ 1.39
26.	8 21 0	4 35 37.95				
			24 41 0.43	= 24 36 58.37	24 37	+ 1.58
27.	8 58 0	5 16 38.38				

THOMAS GLANVILLE TAYLOR.

Table F.—Vibration of Pendulum 12 at Greenwich, July 1828.

Exp. 1. July 21st. Clock making 86401.87 Vibrations. Barom. {29.38 / 29.37} 29.375

No. of Coincid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc.	Observer.	Mean Interval.	Correct. for Arc.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day at 62°.
	°	m s	m s	h m s	°		s	s	s	
1	.....	14 42	14 44							
2	59.6	21 12	21 15	8 21 13.83	0.90	Mr. Taylor.				
3	.....	27 43	27 47							
21	.....	25 05	25 11				391.27	+0.66	−0.69	85960.18
22	61.2	31 36	31 43	10 31 39.33	0.423	Mr. Taylor.				
23	.....	38 07	38 14							
42	61.6	42 02	42 12	12 42 07	0.215	Capt. Sabine.	391.38	+0.16	−0.25	85960.26
62	62.2	52 23	52 39	2 52 31	0.110	Capt. Sabine.	391.2	+0.04	−0.04	85960.15
		Fresh impulse given.								
1	.....	58 48	58 52							
2	62.3	05 18	05 23	3 05 20.5	0.720	Capt. Sabine.				
3	.....	11 49	11 53				390.675	+0.43	+0.08	85960.06
22	62.1	15 30	15 38	5 15 34	0.340	Capt. Sabine.				
42	61.4	25 43	26 12	7 25 57.5	0.170	Mr. Taylor.	391.175	+0.10	−0.10	85960.13
	61.49									85960.16 at 62°.

Exp. 2. July 22nd. Clock making 86401.65 Vibrations. Barom. {29.43 / 29.50} 29.465

No. of Coincid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc.	Observer.	Mean Interval.	Correct. for Arc.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day at 62°.
	°	m s	m s	h m s	°		s	s	s	
1	.....	26 56.0	26 58							
2	58.5	33 26.0	33 28	8 33 27.1	0.933	Mr. Taylor.				
3	.....	39 56.5	39 58				391.32	+0.71	−1.20	85959.58
22	59.9	43 50.0	43 57	10 43 53.5	0.440	Capt. Sabine.				
42	61.0	54 20.0	54 28	12 54 24	0.215	Capt. Sabine.	391.525	+0.17	−0.67	85959.79
51	61.3	53 00.0	53 11	1 53 05.5	0.170	Capt. Sabine.	391.278	+0.06	−0.36	85959.78
	60.17									85959.70 at 62°.

Table F. (Continued.)

The Pendulum taken down, the planes wiped and their adjustment examined; and the Pendulum replaced.

Exp. 3. July 23rd. Clock making 86401.74 Vibrations. Barom. $\left\{\begin{matrix}29.60\\29.58\end{matrix}\right\}$ 29.59

No. of Coincid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc.	Observer.	Mean Interval.	Correct. for Arc.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day at 62°.
	°	m s	m s	h m s	°		s	s	s	
1	.....	01 13	01 15							
2	59.4	07 44	07 45	8 07 44.33	0.960	Mr. Taylor.				
3	.....	14 13	14 16				391.00	+0.75	−0.82	85959.71
21	.....	11 30	11 36							
22	60.8	18 01	18 07	10 18 04.33	0.447	Mr. Taylor.				
23	.....	24 33	24 39				391.31	+0.18	−0.35	85959.98
42	61.6	28 26	28 35	12 28 30.5	0.215	Capt. Sabine.				
62	62.3	38 48	38 57	2 38 52.5	0.110	Capt. Sabine.	391.10	+0.04	−0.02	85959.92
		Fresh impulse given.								
1	62.3	46 22	46 25	2 46 23.5	0.990	Capt. Sabine.				
21	62.3	56 26	56 32	4 56 29	0.480	Capt. Sabine.	390.275	+0.84	+0.13	85959.95
	61.45									85959.89 at 62°.

Exp. 4. July 24th. Clock making 86401.54 Vibrations. Barom. $\left\{\begin{matrix}29.55\\29.50\end{matrix}\right\}$ 29.525

No. of Coincid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc.	Observer.	Mean Interval.	Correct. for Arc.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day at 62°.
	°	m s	m s	h m s	°		s	s	s	
1	.....	40 50	40 52							
2	61.1	47 20	47 22	8 47 21.5	0.950	Mr. Taylor.				
3	.....	53 52	53 53				390.725	+0.75	−0.35	85959.68
22	61.3	57 34	57 38	10 57 36.0	0.470	Capt. Sabine.	391.125	+0.19	−0.19	85959.73
42	61.8	07 55	08 02	1 07 58.5	0.215	Capt. Sabine.				
62	62.2	18 15	18 25	3 18 20.0	0.110	Capt. Sabine.	391.075	+0.04	.....	85959.71
		Fresh impulse given.								
1	62.3	25 18	25 22	3 25 20.0	0.750	Capt. Sabine.				
21	62.5	35 32	35 37	5 35 34.5	0.345	Mr. Taylor.	390.725	+0.46	+0.17	85959.91
41	62.3	45 50	46 06	7 45 58.0	0.165	Mr. Taylor.	391.175	+0.10	+0.17	85960.07
	61.93									85959.82 at 62°.

Table F. (Continued.)

Exp. 5. July 25th. Clock making 86401.39 Vibrations. Barom. $\left\{\begin{matrix}29.45\\29.50\end{matrix}\right\}$ 29.475

No. of Consid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc.	Observer.	Mean Interval.	Correct. for Arc.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day at 62°.
	°	m s	m s	h m s	°		s	s	s	
1	61.0	19 46	19 47	6 19 46.5	0.980	Mr. Taylor.	391.175	+0.78	−0.32	85960.11
21	61.5	30 08	30 12	8 30 10.0	0.440	Mr. Taylor.	391.40	+0.18	−0.14	85959.95
41	61.8	40 34	40 42	10 40 38.0	0.225	Capt. Sabine.	391.20	+0.04	+0.17	85959.88
61	63.0	50 56	51 08	12 51 02.0	0.110	Capt. Sabine.				
		Fresh impulse given.								
1	63.0	57 41	57 44	12 57 42.5	0.910	Capt. Sabine.	390.85	+0.67	+0.39	85960.34
21	62.8	07 56	08 02	3 07 59.5	0.430	Capt. Sabine.	391.025	+0.18	+0.29	85959.93
41	62.6	18 16	18 24	5 18 20.0	0.235	Mr. Taylor.				
	62.24									85960.04

Exp. 6. July 26th. Clock making 86401.58 Vibrations. Barom. $\left\{\begin{matrix}29.63\\29.65\end{matrix}\right\}$ 29.64

No. of Consid.	Temp.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc.	Observer.	Mean Interval.	Correct. for Arc.	Reduct. to 62°.	Corrected Vibrations in a Mean Solar Day at 62°.
	°	m s	m s	h m s	°		s	s	s	
1	60.2	47 31	47 33	6 47 32.0	1.005	Mr. Taylor.	391.05	+0.88	−0.65	85959.86
21	60.8	57 51	57 55	8 57 53.0	0.465	Mr. Taylor.	391.50	+0.19	−0.29	85960.12
41	61.8	08 19	08 27	11 08 23.0	0.220	Mr. Taylor.	391.65	+0.04	+0.02	85960.46
61	62.3	18 52	19 00	1 18 56.0	0.115	Mr. Taylor.				
		Fresh impulse given.								
1	62.3	39 56	39 58	1 39 57.0	0.995	Mr. Taylor.	391.225	+0.35	−0.06	85960.18
61	61.4	11 03	11 18	8 11 10.5	0.110	Mr. Taylor.				
	61.63									85960.15

X. *On a definite arrangement, and order of the appearance and progress, of the Aurora Borealis; and on its height above the surface of the earth. In a letter to* DAVIES GILBERT, *Esq. M.P. President of the Royal Society. By the Rev.* JAMES FARQUHARSON, *Minister of the Parish of Alford, Aberdeenshire.*

Read January 22, and February 29, 1829.

MAY I take the liberty of addressing to you, as President of the Royal Society, a request that you will communicate to the Society, if you shall deem them worthy of its notice, the observations on the aurora borealis which I am about to detail in this letter, and which have been made by me at various times under very favourable circumstances, arising out of the very frequent occurrence of the meteor in the latitude of my residence, about 57° 15′ N.

The immediate occasion of this communication is the interest you have manifested in the subject, in your observations on a luminous belt seen at Rosemorran in Cornwall, 29th September last, published in the Phil. Mag. vol. iv. page 453*; and likewise a paper by JOHN DALTON, F.R.S., published in the Phil.

* Mr. GILBERT's remarks referred to in the text are as follows:

"The luminous belt which exhibited itself on the evening of September the 29th, in the present year, having been noticed and described from various parts of England, I beg leave to communicate its position as observed at a point very distant from most of the other stations, and therefore likely to be affected by a considerable variation of parallax.

"I was then at Rosemorran, the seat of GEORGE JOHN, Esq., an elevated situation near Penzance, twelve miles from the Land's End. My attention was called to this unusual phænomenon at about eight o'clock. The belt then appeared to rise from the horizon, somewhat to the southward of west, and ascended with a steady light and uniform subtense, of perhaps three degrees, towards the zenith, passing over various stars that were scarcely altered in their appearance, till it reached Alpha Lyræ, then somewhat south of west, and nearly 62 or 63 degrees high. From thence diminishing in brightness it became soon blended with the milky way, and ceased to be distinguishable. The belt seemed exactly similar to a ray of the northern light, except that not the least coruscation was to be observed. Its position could not be much out of the magnetic equator.

"Sir WILLIAM ELFORD, F.R.S. has favoured me with a detail of the appearances seen near Totness, very much agreeing with the above statements."

Trans. 1828, "On the Height of the Aurora Borealis above the Surface of the Earth; particularly of one seen on the 29th of March, 1826." Mr. DALTON in that paper, supposing the same luminous belt was seen on the 29th of March at places distant from each other at the same time, infers its height to have been 100 miles and upwards.

If I shall have occasion to differ widely from Mr. DALTON's conclusions, I beg to do so in terms of high respect for that distinguished individual, whose labours have so much benefited science; and whose opinions I should not have ventured to controvert, had I not possessed peculiar advantages for observation; and had I not made on one occasion an observation which appears decisive of the question of height, as will be afterwards stated.

I do not mean to confine myself, however, to the discussion of that question only, but to communicate several very curious results of the numerous observations I have made, which as far as I am able to ascertain are not yet generally understood among men of science.

I had announced these results in a short paper published in the Edin. Phil. Journ. vol. viii. p. 303, April 1823; they are, "That the aurora borealis has in all cases a determinate arrangement and figure, and follows an invariable order in its appearance and progress;—that the streamers (pencils of rays) of the meteor generally appear first in the north, forming an arch from east to west, having its vertex at the line of the magnetic meridian;—that when this arch is yet only of low elevation, it is of considerable breadth from north to south, having the streamers of which it is composed placed cross-wise in relation to its own line, and all directed towards a point a little south of the zenith;—that the arch moves forward towards the south, contracting its lateral dimensions as it approaches the zenith, and increasing in intensity of light by the shortening of the streamers near the magnetic meridian, and the gradual shifting of the angles, which the streamers near the east and west extremities of the arch make with its own line, till at length these streamers become parallel to that line, and then the arch is seen as a narrow belt, 3° or 4° only in breadth, stretching across the zenith at right angles to the magnetic meridian;—that it still makes progress southwards; and after it has reached several degrees south of the zenith, again enlarges in breadth, by exhibiting an order of appearances the reverse of that which had attended its progress towards the zenith

from the north;—and that the only conditions that can explain and reconcile these appearances are, that the pencils of rays (streamers) of the aurora borealis are vertical, or nearly so, and form a deep fringe, which stretches a great way from east to west at right angles to the magnetic meridian, but which is of no great thickness from north to south; and that the fringe moves southward, preserving its direction at right angles to the magnetic meridian."

In the paper from whence these results of observation are quoted, I had not entered into a minute detail of any individual observations, but had satisfied myself with a general description of an order in the appearance and progress of the meteor, which I had repeatedly watched; and a brief account and explanation of some of the apparent irregularities; hoping this might be sufficient to direct other observers to watch likewise this remarkable order. I had also not distinctly stated, although it was to be inferred from some parts of the description then given, that several successive arches of aurora often appear at the same time within the field of view; a circumstance of great importance when considered in reference to the numerous observations so industriously collected by Mr. Dalton.

As I am aware the Royal Society justly prefers details of separate observations to any more general descriptions, I shall now give an account of two or three out of several observations I have had opportunity for making since 1823; the results of which have been all confirmatory of the above views, with very trifling modifications. Regarding the observations I had previously made, and which opened up to me such peculiar views, I shall only now state, that it was in the autumn of 1814 that I first distinctly observed the ordinary aurora borealis, of long vertically-directed streamers, fairly make its progress from a low northerly situation onward to the zenith, and assume there the form of a narrow luminous belt, at right angles to the magnetic meridian. The discovery inspired me, at the time, with a high degree of satisfaction; as the apparent general confusion and wild irregularity of the aurora, when viewed in connection with the peculiar circumstance of its most frequently presenting itself in all localities in some determinate relation to the magnetic meridian, admitted now of easy explanation; and a determinate arrangement and figure, and constant order in the progress of the meteor, to my mind instantly became certain.

On the evening of the 22nd of November, 1825, when returning to my own

house on foot, at half-past ten o'clock, I saw a fine display of the aurora borealis, accompanied with peculiar circumstances, to be afterwards detailed, which would scarcely leave a doubt respecting the elevation of the region which it occupied above the surface of the earth. These peculiar circumstances arrested my attention, and led me to observe every thing in its appearance and progress. When first seen it had already formed two distinct and separate arches in the north and north-eastern parts of the heavens, the continuity of each of which was only interrupted by a few detached masses of low clouds, coming, with a gentle breeze, slowly from the north, and brightly illuminated by the moon. The most southerly arch approached within about 25° of the zenith. It was abruptly terminated at its west extremity, about 35° above the horizon; as will be afterwards more particularly described in discussing the question of the height above the surface of the earth. This west abrupt extremity was a little to the north of west. Its east extremity was near the horizon in north-east, nearly as I could judge at the time. The streamers at the vertex of this arch were very short and compact, and parallel to the magnetic meridian. From this point, towards both extremities, the streamers gradually increased in length, and being all directed to a point apparently 10° or 15° south of the zenith, all formed angles with the general line of the arch, which were more acute in proportion to the distance from the vertex. The arch might be about 10° broad, and speedily moved southward, maintaining a parallelism with its first position. Its lateral dimensions became gradually contracted. The streamers near the zenith shortened into dense bundles, like sheaves of light, parallel to the magnetic meridian, and consequently at right angles to the general line of the arch; and those towards the extremities gradually diminished the angles which they made with that line, and approached to a parallelism with it. At length after reaching the zenith, the arch became diminished in breadth to about 3° or 4°, and coincided in its whole extent with the prime vertical to the magnetic meridian; and the light at its vertex exhibited a nebulous or mottled appearance, and that of the extremities of long streamers or pencils of rays, now parallel to the arch itself. I had no opportunity, upon the present occasion, to witness the enlargement of the breadth again, and the unfolding of the parallel streamers at the vertex, which I had observed in former arches when they got considerably beyond the zenith; for

this arch gradually faded and became extinct, about 10° or 12° southwards of the zenith.

The other arch of this evening was in its general outline parallel to the one now described, but much lower in the heavens. It also ended abruptly at its western extremity, on a point of the compass much nearer the north than the termination of the other. Its vertex was on the magnetic meridian raised about 25° or 30° above the horizon; and its eastern extremity ended at the horizon considerably to the eastward of north. In breadth it at first occupied a space of probably 15° or 20°; but this could not be correctly estimated, as its northern and southern edges were very irregular and variable, owing to the incessant shortening and lengthening of the streamers of which it was composed, which were nearly vertical, and therefore at right angles to the general direction and line of the arch. This was its first appearance; but it gradually rose in the heavens, and became much enlarged both in length and breadth, increasing the azimuth distance of its extremities from the north, as its vertex rose higher in the heavens, but still remaining abruptly broken short at the western extremity, and lengthening its vertical streamers till its middle part reached an elevation of about 45°. In the mean time, however, the streamers near the extremities were gradually changing the angle they made with the line of the arch, constantly directing themselves, whatever position the arch was in, to a point somewhat south of the zenith. After passing the elevation of 45°, the streamers again gradually became shorter at the vertex; and those at the extremities going on at the same time to diminish their angle with the line of the arch, the breadth of that became at all points gradually less. This order of appearances went on till this arch had attained nearly the first position and appearance of the one first described, when it rather suddenly became extinct about fifteen minutes after the other had disappeared.

A luminous space near the north point of the magnetic meridian, appeared at this time to promise the formation of a third arch, beginning to show a few streamers; but it soon gradually faded.

On the evening of the 9th of September, 1827, at 11 o'clock, I witnessed a very brilliant, and, with the exception of the one last described, the most instructive aurora borealis which it has fallen to my lot to observe. When first seen, a bright arch of light of various width, and jagged at its edges, stretched

across the heavens from east to west, about 8° or 10° north of the zenith; its western end resting on a low cloud, and the eastern one descending with a full rich light close to the visible horizon. As this was the first time I had seen such an elevated arch reach to the horizon, that circumstance, and some others, excited my attention particularly. The eastern extremity of the arch was of unusual breadth, about 20°, for about the same number of degrees upwards,—seeming to falsify the conclusions, which I had derived from former observations, regarding the thinness of the fringe of vertical rays. Another arch appeared further north, about 40° high, and 20° or 25° broad, composed of streamers all directed to a point south of the zenith. This last had the peculiarity of being suddenly bent downwards, and narrowed in its western part from a point near the magnetic meridian. The horizon near the magnetic meridian was at the same time brightly illuminated under the arches.

A quick progress towards the south became immediately sensible in both arches. The highest arch in a few minutes reached the zenith, and appeared there much narrower and better defined in its edges; and when at this height, its eastern broad end became resolved into two separate and nearly vertical columns of light, the most southerly of which was the continuation of the arch itself, and the most northerly a low column of about 20° in height. Each of these columns, when in their progress south they attained their narrowest dimensions, was about 5° broad, and the interval which separated them a little more. Here then were two vertical fringes of rays, following each other at a comparatively short interval; or what is rendered more probable, from a circumstance which will be presently stated regarding the more northerly arch, two fringes, preserving parallel planes, and simultaneously moving southward, but the one placed both to the north and the east in relation to the other. This arch was both broader and more irregular in its edges at the zenith than any I had formerly seen, being at least 6° in breadth. Its progress southward was very evidently occasioned by the formation of patches, and even very narrow parallel zones of light at its southern edge, sometimes considerably beyond its line, and the extinction of patches at the northern edge*. Its progress southward was very quick, ranging over 40° in about ten minutes. It reached about 30° south of the zenith, and then slowly disappeared; previously to which, how-

* I have often in other cases seen this peculiar manner of progress.

ever, it had much increased in breadth, exhibiting at the same time a renewal of short streamers at its vertex, parallel to the magnetic meridian, and streamers placed angularly to its own line near the extremities, but now increasing their angles in a reversed order, and still directed lengthwise to a point somewhat south of the zenith.

The more northerly arch in the mean time gradually came forward towards the zenith, exhibiting in succession the appearances which I have already described. When it had attained a high elevation, its western bent end became resolved into two abrupt portions of arches, occupying planes parallel with that of the eastern end; but both of them thrown back in relation to the eastern end, and the one in relation to the other, by a distance of 6° or 8°, in the manner of troops in echelon. The eastern part of this arch had reached very near the zenith, when it suddenly disappeared, and the two westerly fragments gradually faded and became extinct. Both these arches were like the others at right angles to the magnetic meridian.

By this time the light at the northern horizon was greatly enlarged, and soon assumed the form of a new arch of vertical streamers, raised at its centre a few degrees above the horizon; but it did not continue long. After it disappeared, a narrow space at the magnetic meridian again became bright, but the observations were discontinued. On this evening there was a gentle south-westerly breeze, carrying before it a few detached clouds; and an extensive low cloud rested on the tops of the remote mountains in the west. The moon shone brightly; but the light of the meteors literally contended with the power of her beams.

Lest this letter should be extended to too great a length, I shall now only briefly state the observations of one other evening, that of the 29th of September 1828, which may be interesting to you as contemporaneous with your own observation of a luminous arch at Rosemorran.

About ten minutes before eight o'clock I first noticed a remarkable appearance of aurora, and called out some persons then at my house, to witness it with me. At this time the meteor appeared dispersed irregularly into every quarter of the heavens, chiefly in the form of groups of unusually long streamers, all however directed to a point a little south of the zenith; and I had the satisfaction, for the first time, of seeing streamers near the southern as

well as near the northern horizon. The experience derived from former observations taught me to discern immediately an arrangement of these groups, which formed fragments of at least three separate fringes. Observing several groups approaching, from the north, the prime vertical to the magnetic meridian at both its ends, I requested the persons in company with me to watch for a little, when they would see two very brilliant and narrow columns of light at these two quarters, which I pointed out to them. This we all soon witnessed; but the arch over the zenith was not complete, from a deficiency of streamers there. Further observations were not at this time continued. I have since received information that similar appearances were observed at the same time in various parts of this county; and I refer to my own observations of this evening as a proof of the justness of the conclusions, I had formerly arrived at, regarding the peculiar arrangement and progress of the meteor, as well as showing, in conjunction with your observation in Cornwall, that the peculiar matter of the meteor was contemporaneously very active at nearly the extreme north, as well as the extreme south of this Island.

The observations, now briefly detailed, all evidently go to confirm the correctness of the views I had announced in 1823; and the only modifications of the statements then made, now directed by them, are two of a very trifling description. The one, that the point, to which the streamers are directed, is a little more south of the zenith than I had then estimated it. The uncommonly long and brilliant streamers of 9th September 1827, and 29th September 1828, led to this correction. I would now estimate the point at 15°, or rather more south of the zenith, instead of 10°: but objects so evanescent and unsteady do not admit of preciseness in making such an estimate; and there may yet be an error of 3° or 4°. The other modification is, that the luminous belt is, at the zenith, sometimes a little broader than I had stated it at its maximum (5°); and this correction is directed by the appearance of the most southerly arch of 9th September, 1827, which moved southward with unusual rapidity, and appeared about 6° broad. There is now likewise an addition to be made to the former statements, which is, that the extremity of the zenith arch sometimes descends to the horizon.

I am fully aware how important it is in the eyes of the Royal Society, that the observations of one individual, of any natural phænomena, should be fully

verified by others; and it is a fortunate circumstance that those which I have now detailed are almost all incidentally verified by some or other of the many observations collected by Mr. DALTON, although evidently none of these have been made under the impression of those views of the peculiar arrangement of the meteor, which frequent opportunity for observation had opened up to me. I shall briefly point out the verifications. The references are to Mr. DALTON's paper in the Philosophical Transactions. It is unnecessary to refer specifically to the testimonies to three facts, as almost all the observations coincide regarding them. 1st, That the arch of the aurora, when at the zenith, is placed at right angles to the magnetic meridian: 2nd, That there it is only of small breadth: and 3rd, That it moves southwards. None of the observers appear to have had an opportunity for observing the transition of the vertical streamers from a low northerly situation into the zenith arch; but Messrs. COLDSTREAM and FOGGO at Edinburgh saw, and have well described some of the concluding stages of this transition, and the transformation which the figures and intensity of the lights undergo at the approach to the zenith. The sudden formation of the meteor, only "a few degrees to the north of the zenith," gave them no opportunity for observing the earlier stages. They say; "When first formed, it" (an arch of silver light) "was a few degrees to the north of the zenith of this place; the light in the centre was rather diffuse; its edges were irregular; and the western limb had, as it were, a plumose appearance. It soon evinced a decided motion towards the south, and in a few minutes reached our zenith. Its edges were now sharply defined, and throughout its whole course it was nearly uniform in appearance and breadth; the intensity of light in its zenith had increased, while in the same quarter the breadth had considerably diminished." These terms might be interchanged with the description of this last stage of the transition, given by me in 1823.

The enlargement of the breadth of the arch after it has passed considerably to the south of the zenith, is verified by the observer at Newton-Stewart: "It was a bow or arch of silvery light, stretching from east to west, and intersecting the meridian at a few degrees to the southward of the zenith; after expanding a little in breadth and shifting for a short way further to the south, it disappeared." Unfortunately no notice is taken of the breadth of the arch as seen at Jedburgh, 30° and ultimately 50° south of the zenith: but the ob-

server there confirms the revival of the flitting streamers at these angles south; for he says, "waves of light seemed to run along the arch*."

The contemporaneous existence of two or more arches within the field of view, is fully verified by Mr. SAMUEL MARSHALL, who saw a zenith arch, and another parallel, about 20° north of the former, "of less intense light;" and the northern horizon was luminous. Many of the reports also state, that, contemporaneously with the zenith arch, there were ordinary auroræ in the north.

It should not be overlooked, as a circumstance inferring the fringe-like form

* I have been desirous, in detailing these verifications, to confine myself chiefly to those found in the observations collected by Mr. DALTON; but I may be permitted to refer to observations of a fine zenith arch, seen at and near Aberdeen on the 15th of September last, as reported in the Aberdeen Journal of the 17th September. I saw the same arch here at the same time with the observers in Aberdeen. It was here considerably south of the zenith, and extended nearly to the west horizon as well as the east; and as this place is twenty-five miles west of Aberdeen, this proves the great reach of the arch from east to west. It moved slowly southward, and faded soon after I saw it, in company with three other individuals. The first report in the Aberdeen Journal verifies the increase of breadth of the arch, and the re-appearance of the streamers when it gains a position considerably to the southward. It is stated in the Journal, that the observer in this case made his observations a little to the eastward of Union Bridge. The following are his terms: "On Monday evening last the rare phenomenon of a luminous arch made its appearance in the sky, commencing about nine o'clock, and continuing for about forty or forty-five minutes. When first noticed it presented a very bright zone of white light, in breadth about 2½°, which it preserved throughout. Its vertex, or most elevated part, was not far to the southward of the zenith; its direction very nearly at right angles to the magnetic meridian, stretching across the whole heavens from E. to W., and having a slow motion southwards, so that it moved through a space equal to its own breadth in about ten minutes, leaving to the north the constellation Lyra, part of which it had previously included. It terminated at each end near the horizon, behind low masses of stationary clouds, of the species denominated stratus; its eastern leg was not so straight as the western, but had a sensible inclination towards the N., bending at a very obtuse angle. The evening was clear and calm, the stars bright, Barom. 30.31, Therm. 48°. The whole arch was of nearly uniform brightness, except near the extremities, where it became somewhat fainter. There were no sensible motions or coruscations of light; the larger stars appeared through it, and two shooting stars were seen to fall from neighbouring parts of the sky. About half-past nine it was breaking up slowly, becoming at first broader in the western leg, and then spreading into thin vertically disposed streams of light, which faded gradually away."

The second report is from a station about two miles north of Aberdeen. It is much more brief, and differs from the first, in stating, that "at a quarter past nine the arch was complete from the eastern to the western horizon without intervening clouds." Aberdeen Journal, 17th September, 1828, p. 3.

of the vertical arch, that some reporters, quoted by Mr. Dalton, state the light towards the extremities to be more intense than that at the zenith. In this fringe-like arrangement a line from the eye, directed considerably towards the East or West, would penetrate and collect the light of many parallel streamers; whereas one directed to near the zenith would, on account of their vertical position, penetrate only a few.

I now proceed to the question of the height of the aurora borealis above the surface of the earth. In the paper in the Edinburgh Philosophical Journal, 1823, I had inferred, from the bright phosphorescent light of a cloud apparently under an aurora, that they were in contact, or nearly so, with each other. Another similar appearance, of a still more decided character, in the autumn of 1825, but the precise date of which I have not noted, confirmed, in my mind, the justness of the inference. In a dark evening, without moon, an extended mass of clouds stretching along the N. and N.E. quarter, not much raised above the visible horizon, and having a clear sky above it, in which there was playing a fine aurora of vertical streamers, with their lower extremities apparently touching it, was observed giving out at its upper side a fitful but bright white light, more vivid and conspicuous amidst the darkness than if it had been illuminated by the rising moon. Similar clouds in other parts of the horizon exhibited no such light. It was impossible for a spectator to refer the aurora to a distance more remote than that of the mass of clouds; or to believe that the former and the light of the latter were not parts of the same phænomenon. Mr. Otley (Phil. Trans. *l. c.*) appears to have witnessed a similar phænomenon. "About 7 P. M. a dense cloud appeared in the horizon to the N.N.W., bounded by a bright line, the rest of the heavens being starry. Presently beams of an aurora began to shoot towards the Great Bear." The appearances now mentioned are of a nature to admit, probably, of frequent verification.

But a combination of circumstances attended the aurora borealis, as seen by me on the 22nd of November 1825, and described above, which seem decisive of this question. On that evening, besides the small detached clouds of the eastern and zenith part of the heavens coming slowly from the north, another of a quite different character extended along the whole western part of the sky to about 25° or 30° above the horizon. It was one dense sheet or stratum,

comparatively, with the other clouds, very dark below, waved or furrowed from north to south, and cut off at its east side in an apparently straight edge, trending nearly north and south. It was coming on very slowly towards the east, and had before next morning prevailed over the other clouds, covering the heavens, and accompanied with a fresh westerly breeze, after a frosty night which the 22nd of November was. This large sheet of cloud was much more elevated than the small detached ones, as was fully proved by some of the latter being projected, in perspective, on its dark under surface, and there appearing as white masses fully enlightened by the moon.

Now the two arches of aurora of that evening were abruptly terminated at the points where they appeared over the eastern edge of the large cloud; and the abrupt terminations increased their azimuth distances from the north, as the arches came southwards, still appearing, in their new positions, over the east edge of the cloud. The lower extremities of the streamers, which were as long at these terminations as at any other parts of the arches, appeared even in contact with the cloud; and I sometimes conceived that they stretched before its eastern edge: but that part being considerably illuminated by the moon, prevented me from being quite positive. Independently, however, of this uncertainty, the appearances are surely decisive of the fact, that the aurora did not extend into the region occupied by the western cloud; and being seen over it at an angle not much higher than its own, occupied therefore a region of nearly equal elevation above the surface of the earth.

The conclusions to be drawn from these observations harmonize sufficiently with an observation of Capt. Parry and Lieuts. Sherer and Ross, who, at Port-Bowen on the 27th of January, 1825, simultaneously saw "a bright ray of the aurora shoot suddenly downward from the general mass of light, and between them and the land, which was then distant only three thousand yards." (Journ. of a Third Voyage under the orders of Capt. William Parry, R.N. F.R.S. 1826.) The conclusions are, that the region occupied by the aurora borealis is immediately above, and contiguous to, the region in which aqueous vapour is forming, or about to be formed, in the shape of clouds. The real height will of course vary with the different states of the atmosphere; I should not have estimated the height of the phosphorescent clouds, above described, at so much as two thousand feet above the surface, or twice the height of some of the

neighbouring hills; but while the lower ends of the vertical streamers were at this height, their upper might be two or three thousand feet more. I have seen the aurora, however, when the clouds certainly occupied a much more elevated region.

I may now be permitted to make some observations on Mr. DALTON's deductions regarding this question: and here I feel that I ought to be very brief, as that gentleman may be disposed now to review them himself, for which he is infinitely better qualified. He will perhaps now allow, that the more common streamers of the aurora, and the zenith arch, are not distinct, although contemporaneous phænomena, as he seems to suppose; but that they are exactly the same thing;—that which is the zenith arch to one set of spectators, becoming resolved into common streamers to other spectators who are placed at some distance, either to the southward or northward of the former. And he may now admit the contemporaneous existence of several parallel arches, even within the same field of view.

Would not the numerous observations made on the 29th of March, 1826, from Edinburgh to Warrington, be more easily explained and rendered consistent with each other, on the supposition, that there were several nearly vertical fringes of the aurora, almost contemporaneously hanging over many lines from Edinburgh to Warrington, at a few thousand feet above the surface of the earth? Are there not even some circumstances, of the numerous observations, that do not admit of being reconciled, on the supposition that only one arch was seen? Thus, for instance, the arch over Edinburgh was seen a few degrees north of the zenith, at $8^h\ 15^m$ P.M.; that at Jedburgh, 30° south of the zenith, exactly at the same hour. These two observations appear quite at variance with each other, if the same arch was seen by Mr. OTLEY at Keswick, at 8 P.M., a little south of the zenith, and by Mr. HOLDEN, at Whitehaven, at $8^h\ 45^m$, 15° south of the zenith:—and the observations at all the four places again become irreconcileable among themselves, if the same arch passed through the zenith, at Kirkby Stephen at 9 P.M. and through the same point at Lancaster at 8 P.M. There are discrepancies also regarding the appearance of the arch itself, in respect of luminousness at its different extremities.

On the other supposition, there would be scarcely any discrepancy. One fringe of streamers might hang over Edinburgh; another nearly over Jedburgh

and Hawick; at which places the fringe at Edinburgh, forty miles distant, with high intervening land, might well be supposed, if only a few thousand feet above the surface of the earth, not to have been sufficiently bright to excite attention; and accordingly it is not stated, that at Jedburgh or Hawick any northern light was seen. The prolongation of a line a little south of Jedburgh and Hawick, at right angles to the magnetic meridian, would pass very near Dumfries; and the fringe in this line might present in a "sky very clear" "a few streamers, low in the horizon," to Mr. HARRIS at Cockermouth, about thirty miles distant, across the valley of the Solway.

A comparison of the times and elevations leads to the inference, that only one fringe was seen at Cockermouth, Keswick, and Whitehaven; and this fringe, when vertical over Keswick, might present the "splendid light that was observable in the northern horizon" at Kendal, about twenty miles distant. The same fringe or the eastern part of it might, in its progress southward, hang over Kirkby Stephen at 9 P.M.

A fourth distinct fringe might hang over Kendal between 8 and 9 o'clock; and a fifth over Lancaster, twenty miles further, at 8 o'clock;—beyond which, as there are no particulars from Preston, the phænomena cannot be compared with each other.

There may, however, have been more fringes than these: but if there were not, the circumstance would well account for there being no reports of similar arches, seen the same evening, at many intermediate places of note, where the arches reported would be resolved into common streamers, and so excite little attention.

Does there not arise an objection to Mr. DALTON's conclusion, that the arch is one hundred miles high, from the circumstance that the light is often so brilliant at the horizon,—as seen for instance by yourself on the 29th of September, and many others,—at various times? Were the arch one hundred miles high, horizontal rays, coming from the lowest part of it, would enter the atmosphere at nearly six hundred miles from the observer, and would have still about two hundred miles of air to penetrate, after they had come within five miles of the earth; without taking into account the refraction, which would increase the distance considerably. Would not the light, therefore, considering that it is at best but a relatively feeble one, be liable to a great or even total obscura-

tion, near the horizon, if coming from a meteor so high. On the other hand, —on the supposition that the meteor is only a few thousand feet above the surface of the earth,—any objection of this nature almost entirely vanishes.

I shall again briefly return to the more general inquiries.

Several of the observations of Captain Parry at Port Bowen support other details in this paper. Thus, at that place, where the variation is 123° 22′ W., and the dip 88° 1′, he most frequently saw the meteor in the form of a low arch from about W. to S.E., more frequently bisected by the plane of the magnetic meridian, than that of the true; and he describes the streamers as vertical, or in the plane of the dip. They are here too in the plane of the dip, or nearly so; since in all situations they direct themselves to a point that appears upwards of 15° south of the zenith.

Will it not now be admitted as proved by the above observations, and their extensive verifications by so many different persons,—that the aurora borealis always presents itself in definite and very curious relations, to the lines of magnetism, indicated by the needle.

That the streamers, in the direction of their length, coincide with the plane of the dip of the needle, or nearly so; and that each individual streamer is, in fact, parallel to the dipping needle.

That they form a thin fringe, stretching often a great way from E. to W. at right angles to the magnetic meridian.

That the fringe moves away from the N. magnetic pole, by the extinction of streamers at its northern face, and the formation of new ones contiguous to its southern face.

That the invariable regularity of its appearance, as seen by so many observers, when it comes fully within command of the eye, near the zenith, shows the apparent irregularities, when it is seen either more northerly or southerly, to be only optical illusions.

And that the region which it occupies is above and contiguous to that of the clouds, or that in which they are about to form?

I had stated in the paper sent to the Edin. Phil. Journ., that the meteor precedes or accompanies westerly and south-westerly gales; but of this there was unfortunately a misprint of south-easterly for south-westerly. It is, indeed, at the period of the westerly equinoxial gales that it is most frequent; and when

it is seen in winter, it is either immediately before, or during the continuance of, a westerly fresh wind.

I would state, in conclusion, that I have here seen it much more frequently in the form of a light near the northern horizon, than in any other form: and, with the views I have now detailed, entertain, therefore, a suspicion that there is some line, near the shore of the Moray Firth, about thirty miles north, or between this place and that, where it oftener forms a zenith arch than here. This might be worthy of inquiry; and should it be found to be so, it might be further worthy of inquiry,—whether the circumstance, considering the attendance of the meteor on westerly gales, may not be dependent on the facility with which these gales can traverse into the Moray Firth, through the deep valley of the Caledonian canal. By such inquiries we might ascertain other relations which it may bear to the various thermometrical and hygrometrical states of the atmosphere.

JAMES FARQUHARSON.

Alford, Aberdeenshire,
Dec. 23rd, 1828.

POSTSCRIPT.

Since the foregoing pages were written, I had, last evening (December 28th) an opportunity, in company with another person, for observing an aurora borealis, which, from several circumstances attending it, may be worthy of description. It was first observed about half past 6 o'clock, in the form of a very complete arch of pale silvery light, nearly uniform in appearance from end to end, the vertex of which was 25° or 30° above the N. horizon at the magnetic meridian; its own breadth 10° or 12°; the light nearly steady, and gradually shaded off at both edges. At the same time a much brighter confined light appeared, close to the horizon, at the N. point of the magnetic meridian. The arch of pale light moved southward very slowly; and after rising 6° or 8° higher, became resolved into pale flitting streamers, which separated into groups, and soon faded, with the exception of those at the E. end, which continued a considerable time, and approached near the prime vertical to the magnetic meridian before becoming extinct. In the mean time, the light at

the horizon in the N. rose upwards with greater quickness, becoming an arch, and in a few minutes attained the first elevation of the one first described; but was much more brilliant at its lower side, where the light had a nebulous and nearly uniform appearance, its upper side being shaded off gradually in the form of pale vertical streamers: and as at this part of its progress some of the streamers of the preceding arch remained still visible, one who had witnessed them for the first time in this position might have taken both for parts of the same arch, and so have been liable to draw very false conclusions regarding the arrangement. The second arch soon expired also near the elevation of 25° or 30°; but not till a third one had risen from the horizon as it had done, forming a still narrower and more brilliant zone of light. This attained speedily a like ultimate elevation with its immediate predecessor; and as it came forward to this situation, two more arches equally narrow and brilliant, rose up in succession under it; so that three low parallel arches were seen, at the same time, in the northern part of the sky, each having its vertex at the magnetic meridian, and each having both its extremities at the horizon. The whole became gradually extinct about 9 o'clock.

These five arches were all unusually flat, extending further both to E. and W. than any I had seen before of equal elevation at the vertex; seeming thus to indicate that the fringes of streamers were of comparatively low elevation above the surface of the earth. The last four were also unusually narrow from side to side, indicating that the streamers were short.

The evening was calm and frosty, and the sky cloudless, during the continuance of the aurora. Afterwards, about half past 10 o'clock, many low clouds were seen in the region which had been occupied by the aurora, moving rather quickly from the west, the rest of the sky continuing at the time cloudless. The following morning there was a fresh westerly breeze, with the thermometer 46° Fahr., which continued through the day. These circumstances lead me to suspect that the aurora was vertical over a valley five or six miles N. of this place, whose longitudinal direction is E. and W.; and through which I have often seen the clouds driven from the west, with much velocity, at the commencement of a westerly gale, several hours before the gale was felt here, where a lofty ridge of hills shuts up the west side of our valley.

I should add, that, although cloudless, the atmosphere was hazy near the horizon at the time of the aurora; Alpha Lyræ being much obscured, and the other two stars of the triangle being scarcely visible. Yet the light of the aurora was very bright close to the tops of the hills.

J. F.

Alford,
December 29th, 1828.

XI. *Observations on the functions of the Intestinal canal and Liver of the human Fœtus.* By Robert Lee, *M.D., Physician to the British Lying-in-Hospital. Communicated by Dr.* Prout, *F.R.S.*

Read June 19, 1828.

WHILE investigating the mode of developement of the organs in the human fœtus, at the different periods of utero-gestation, I was struck with the appearances which were uniformly met with in the contents of the intestinal canal. As these appearances have only been vaguely noticed by physiologists, and as they seem to throw light on some obscure processes of the fœtal œconomy, I have been induced to offer the following account of my observations on this subject to the consideration of the Royal Society.

It has not yet been determined what are the organs which are first developed in the human fœtus, but it is certain that the liver and intestines are visible at a very early period, that these organs are copiously supplied with blood, and that, during the whole period of gestation, they occupy a large portion of the abdominal cavity. The pancreas and spleen can also be distinctly perceived between the second and third month after conception, but, unlike the liver and intestines, they are very sparingly supplied with blood, and remain small and imperfectly developed during the existence of the child in utero. In all the intermediate periods, from the fourth to the end of the ninth month, the small intestines are much more vascular than the stomach and great intestines. The mucous membrane of the upper portion of the small intestines, is of a bright rose-red colour; while that of the ileum and colon is comparatively pale and bloodless. In a fœtus of eight months, the length of whose body was eighteen inches, the small intestines, when fully extended, measured nine feet six inches, and the great intestines two feet; and from other examinations I have been led to estimate the entire length of the intestinal canal of the fœtus, at the full period, at about eleven feet; the relative proportion to the length of the body being much greater than in the adult.

I have now examined, in upwards of twenty fœtuses of different ages, the contents of the different portions of this extensive tube; and from their appearance, as well as chemical composition, as determined by Dr. Prout, to whom numerous specimens were submitted for analysis, it will be perceived that they bear a striking analogy to the contents of the alimentary canal of the adult, where the processes of assimilation and absorption are performed.

The stomach of the fœtus I usually found, in these cases, distended, with a semitransparent, ropy, mucous, and occasionally ascescent fluid, without any sensible admixture of albuminous or other apparently nutritious matter.

In the duodenum, and part of the remaining portion of the small intestines, there was uniformly present, adhering closely to the mucous membrane, a semi-fluid matter, found upon examination to possess properties decidedly of an albuminous character, and to have an orange or pink colour. This matter has always been found in greatest abundance around the papillary projection, through which the common duct of the liver opens into the duodenum.

In the lower half of the small intestines the quantity of this albuminous matter was greatly diminished, and near the colon it almost entirely disappeared. The colour also of the contents of this lower portion of the small intestines was different from that noticed in the contents of the duodenum, being of a greenish tint, and assuming more and more the characters of the meconium as the distance from the origin of the colon diminished. These different substances were generally found slightly ascescent.

The great intestines were much more distended than the small intestines, and contained throughout a dark green, homogeneous, generally neutral or slightly alkaline fluid, in which no albuminous matter could be detected, and which was consequently excrementitious.

The absence of albuminous matter in the stomach of the fœtus, its invariable presence in the upper half of the small intestines, its gradual diminution as we proceed downwards, and its disappearance in the colon, are circumstances, which, viewed in connection with the great length of the small intestines already noticed, seem to prove that the absorption of some nutritious substance takes place from the intestinal canal in the latter months of gestation, in like manner as it does after birth.

The observation, that the lacteals contain a similar fluid, is sufficient to

render the preceding physiological view a matter of demonstration. In no instance, however, have I succeeded in detecting these vessels in the mesentery of the human fœtus, though I have perceived them distinctly in a calf of seven months. A case has been however recorded by Mons. Adelon, in his work on Physiology, where the lacteals were observed in a child at the period of birth distended with chyle. His words are: "En examinant les vaisseaux du mesentère, dans un enfant qui venait de naitre avec l'abdomen ouvert, on a trouvé ces vaisseaux pleins de chyle*."

My attention was next directed to discover the source of this albuminous matter in the intestines, which I conceived could only be derived from the pancreas, the liver, or the duodenum itself. With respect to the pancreas, that organ remains so small during the whole of the fœtal state, that it would appear incapable of forming so large a quantity of matter as exists in the intestinal canal. On the other hand, the duodenum presents still greater difficulties to the solution of this question; for it seems improbable that this portion of the alimentary canal can perform simultaneously the office of secreting and absorbing the same matter; to say nothing of the anomaly which in this case would take place, of a mucous membrane forming albumen. From having observed in every instance the same orange-coloured fluid, in the small intestines, collected in great abundance near the orifice of the ductus communis choledochus, and taking into consideration the magnitude of the fœtal liver, and the large supply of blood which it receives from the umbilical vein, it appeared to me reasonable to infer that this viscus must be the source of the matter in question. Additional weight was given to this conclusion by having detected, in two instances, in the hepatic duct, the presence of a fluid possessing, not only some of the sensible, but also the chemical properties of that which was found in the duodenum. In general, the hepatic and common ducts of the liver have been found empty, or have contained too minute a quantity of fluid to be collected for chemical investigation; but in the two instances above mentioned it existed in unusual abundance, and was pressed out upon a plate of glass without mixing with the bile of the gall-bladder, a ligature having previously been applied around the cystic duct. This fluid of the hepatic duct was of a light straw-colour, and much less viscid than that

* Adelon, Physiologie de l'Homme. Tom. iv. p. 476.

coating the inner surface of the duodenum; and its properties were, if possible, still more decidedly albuminous than those of the intestines.

I have been led to conclude from these facts, that the function of the fœtal liver is not, as has generally been supposed, that of separating from the blood an excrementitious fluid injurious to the œconomy of the child; at least that such is not its only use, but that it also performs some other important office destined to assist in the nutrition of the fœtus.

It would be superfluous here to enumerate the various opinions which have been entertained by physiologists on the subject of the nutrition of the fœtus in utero; but that of Hippocrates as adopted by Harvey in his great work de Generatione Animalium, requires to be noticed. He observes, "Quinetiam certum est, intra pulli ingluviem (talisque prorsus in omnium embryonum ventriculus cernitur) substantiam quandam, colore, sapore, et consistentia dicto jam liquori persimilem reperiri; eandemque, in ventriculo aliquantulum coctam, lac coagulatum referre; quam etiam, chyli specie, in primis intestinis deprehendimus, inferiora autem intestina excrementis stercoraceis referta sunt. Similiter in viviparorum in fœtibus intestina crassiora consimili excremento replentur, quali eadem, cum lacte vescuntur, abundare cernimus. In ovibus etiam, aliisque bisulcis, manifesta sunt scybala.

"Quid dubitemus igitur affirmare fœtum in utero sugere; et in eo fieri chylificationem, cum ejus manifesta adsint tum principia, tum rejectamenta?"

The fallacy of the opinion of Harvey, and of later physiologists with regard to the source of the nutritious fluid found in the intestinal canal of the fœtus, is demonstrated by the fact, that acephalous children*, and those born with the œsophagus impervious, have not only been perfectly nourished, but in their

* Meckel's Manuel d'Anatomie Generale, &c. Tom. iii. p. 792.

The translators of Meckel's work, Messrs. Jourdan and Breschet, have stated the opinion of Geoffroy St. Hilaire on this subject in the following note:—"Geoffroy St. Hilaire (Monstruosités Humaines, p. 279.) ayant rencontré dans le canal intestinal d'un anomocephale de veritables matières fecales moulées même, et reunies en crottins dans l'intestin post-cæcal, s'est trouvé conduit par ce phenomène à examiner la nutrition propre du fœtus. Il pense que le mucus secrété dans les voies alimentaires, et qui est trop abondant pour ne jouer que le role de fluide lubrifacteur, est l'aliment sur lequel agit d'abord la digestion; que pris d'abord par l'appareil digestif, ensuite par les voies chylifères, il est la source du fluide nutritif, qui afflue ainsi sans cesse dans l'appareil circulatoire, et qui, à chaque passage, eprouve une animalization graduelle. Considerée de la sorte, la nutrition du fœtus

intestines substances have been found similar in character to those contained in the intestines of children, in whom no such malformation had existed

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Note by Dr. PROUT.

The principal chemical facts ascertained by me having been introduced by Dr. LEE in the preceding paper, it only remains that I should briefly state the manner in which these facts were determined.

The most unequivocal test of the presence of albuminous matter that I am acquainted with, is the prussiate of potash assisted by dilute acetic acid, as first recommended by BERZELIUS; and this accordingly was the test on which the chief reliance was placed in my experiments. But the presence of albuminous matter was also satisfactorily indicated by other means; as by heat, by the oxymuriate of mercury, &c. Besides the albuminous matters, however, it may be proper to mention that others were present, to which this term, even in its most extended sense, could not be strictly applied, and for which in the present state of animal chemistry, it was difficult to find a precise term; a large proportion of them appeared to be nearly allied to mucus and bile, though they did not exactly agree with these principles as they occur in the adult state.

I cannot close this note without observing how forcibly I was struck by the close resemblance between the phænomena, as above described, and those presented by the intestinal canal, when the processes of digestion and assimilation are known to be going on; and that I cannot at present conceive any other source from whence the matters in question can be derived, than the hepatic system as supposed by Dr. LEE.

W. P.

se rapprocherait de celle de l'adulte. Cette hypothèse, d'apres laquelle l'ecoulement du mucus serait du à l'irritation des membranes muqueuses par la bile, est ingenieuse, mais peu probable. Elle obligerait, en effet, à admettre que le tube alimentaire exerce deux actions totalement differentes à l'égard du mucus, l'une en vertu de laquelle ce mucus est formé, et l'autre qui a pour but de le transformer ensuite et de le convertir en chyle."

XII.—*Experiments on the modulus of Torsion.* By BENJAMIN BEVAN, *Esq.* *Communicated by the President.*

Read December 18, 1828.

NUMEROUS experiments have already been published on the strength of wood and other substances, as far as regards their cohesion and elasticity; but I am not aware of any extensive table of the modulus of torsion of different species of wood, deduced from experiments conducted upon a proper scale, and with the necessary care.

To supply this defect, and to furnish the practical engineer and mechanic with useful data, and with rules for their application, is the object of the present communication, consisting of a copious table of the results of my experiments, made at various times, and upon substances of considerable variety of dimensions within the ordinary limits of practice.

It is proper to observe, that the various specimens of wood upon which my experiments were made, were sound and dry, except it is otherwise expressed or described, and were in general free from all large knots.

Considerable care was used to obtain correct dimensions of the specimens under experiment, by means of a simple instrument, which answers the purpose of improved callipers, by which the dimensions of the specimens were measured, and read off by a magnifying-glass to the 400dth part of an inch. Previous to trial, each specimen was brought to a prismatic form, as near as could be wrought by the ordinary means, and the dimensions afterwards taken by means of the improved callipers above mentioned, at equal distances; and the mean breadth and thickness thus obtained, were used in the calculations for obtaining the modulus. My experiments were often repeated on the same species of wood, under considerable variations of length, breadth, and thickness; and always with the most satisfactory results; viz. from nine to ninety

inches in length, and from three inches to three tenths of an inch in thickness. Due care was observed to prevent any error in the apparent torsion or twist arising from compression at the ends of the prisms, both at the clamp by which they were fixed, and at the radial lever at which the successive weights were applied; two sources of error which have materially affected former experiments on this subject, in other respects carefully made.

To every specimen under experiment I attached two indexes; one a few inches from the end fixed in the clamp or vice, and the other at a small distance from the attachment of the lever or wheel, where the weight or straining power was applied; and the distance between the two indexes was used as the length for calculating. Another error of less magnitude I have been able to avoid by fixing a pivot or small gudgeon at the supported end, in the line of the axis of the prism, instead of making the lower side or angle of the prism at the supported end the revolving point.

My experiments were made upon prisms of very different proportions as to breadth and depth, viz. from $\frac{1}{10}$th to equality.

In general practice, the square or cylindrical shaft is usually adopted, and as a cylindrical spindle or shaft of $\frac{1}{7}$th more in diameter than the side of a square shaft, will possess nearly the same stiffness in resisting a twisting force, it will, I presume, be sufficient in this place to give the rule for calculating the deflection of a square prismatic shaft, to which I shall add one example by way of illustration.

Rule.—To find the deflection δ of a prismatic shaft of a given length l when strained by a given force w in pounds avoirdupoise acting at right angles to the axis of the prism, and by a leverage of given length $= r$; the side of the square shaft $= d$. T, being the modulus of torsion from the following table; l, r, δ, and d, being in inches and decimals,

$$\frac{r^2 l w}{d^4 \mathrm{T}} = \delta$$

i. e. for a numerator, the square of the radius of the wheel or leverage multiplied into the length, and this product by the weight in pounds: and for a divisor, multiply the fourth power of the side of the square prism by the tabular modulus of torsion: divide the former by the latter, and the quotient will be the deflection or quantity of twisting in inches and decimals when measured at

the end of the radius r. As an example, let there be a square* shaft of English oak 50 inches long and 6 inches by 6 inches, subject to a strain of 3000lbs. at the circumference of a wheel of 2 feet in diameter, or having a leverage of 12 inches†.

$6 \times 6 = 36$ $\qquad$ $12 \times 12 = 144$

36 $\qquad$ $50 = \text{length}$

1296 $\qquad$ 7200

20000 $\qquad$ $3000 = \text{force}$

25920000 $\qquad$ $25920000)21600000(0.83 = \text{deflection},$

or nearly $\frac{4}{5}$ths of an inch. And as the deflection will be directly as the force, a weight or force of 300lbs. would produce a deflection of $\frac{1}{10}$th of an inch.

TABLE of the Modulus of Torsion.

Species of Wood.	Specific gravity.	Modulus of Torsion. Pounds.	Observations.
Acacia	.795	28293	Not quite dry.
Alder	.55	16221	Cross-grained.
Apple	.726	20397	
Ash		20300	Of my own planting.
Ash, mountain . . .	.449	13933	
Beech		21243	
Birch		17250	
Box	.99	30000	Old, and very dry.
Brazil wood	1.05	37800	Old, and very dry.
Cane		21500	Influenced by the hard surface.
Cedar, scented . . .		12500	
Cherry	.71	22800	
Chesnut, sweet . . .		18360	
Chesnut, horse . . .	.615	22205	

* If the transverse section of the prism or shaft be not a square, but a parallelogram, let b = the breadth, and d the depth: the deflection will be obtained by the following formula:

$$\frac{(d+b)\,l\,r^2 W}{2\,b\,d^3 T} = \delta.$$

† If the measure of torsion should be required in degrees (Δ)

let $\rho = 57.29578$ then $\frac{r\,l\,w}{d^4\,t} = \Delta$

or let $\frac{T}{\ell} = t$ then $\frac{r\,l\,w}{d^4\,t} = \Delta$

thus for wrought iron and steel $\frac{r\,l\,w}{31000\,d^4} = \Delta$

cast iron $\frac{r\,l\,w}{16600\,d^4} = \Delta$

Table (Continued).

Species of Wood.	Specific gravity.	Modulus of Torsion. Pounds.	Observations.
Crab	.763	22738	
Damson		23500	
Deal, Christiana	.38	11220	
Elder	.755	22285	
Elm		13500	
Fir, Scotch		13700	
Hazel	.83	26325	Not quite dry.
Holly		20543	
Hornbeam	.86	26411	Not quite dry.
Laburnum		18000	Green, or fresh cut.
Lance-wood	1.01	25245	
Larch	.58	18967	
Lime or Linden	.675	18309	
Maple	.735	23947	Partly cross-grained.
Oak, English		20000	
Oak, Hamburgh	.693	12000	
Oak, Dantzic	.586	16500	
Oak, from Bog	.67	14500	
Ozier		18700	
Pear	.72	18115	
Pine, St. Petersburgh		10500	Fresh.
Pine, St. Petersburgh		13000	Four or five years old.
Pine, Memel		15000	
Pine, American		14750	
Plane	.59	17617	
Plum	.79	23700	
Poplar	.333	9473	
Satin-wood	1.02	30000	
Sallow		18600	
Sycamore		22900	
Teak		16800	Old, and partially decayed.
Teak, African		27300	
Walnut	.572	19784	

I have observed in a great number of my experiments, that the modulus of torsion bears a near relation to the weight of the wood when dry, whatever may be the species; and that for practical purposes we may obtain the deflection (δ) from the specific gravity (s). Thus

$$\frac{r^2 l w}{30000 d^4 s} = \delta.$$

TABLE of the modulus of torsion of Metals.

	Specific gravity.	Modulus of Torsion. Pounds.
Iron, English (wrought).		1810000
Iron, English (wrought).		1740000
Iron, thin hooping. . .		1916000
Steel		1984000
Steel		1648000
Steel		1618000
Iron cylinder		1910000
Iron cylinder		1700000
Iron square		1617000
Iron square		1667000
Iron square		1951000
Mean of Iron and Steel		1779090
Iron, Cast		940000
Iron, Cast		963000
Iron, Cast		952000
Mean of cast-iron . .	7.163	951600
Bell-metal	8.531	818000

On comparing these numbers with the modulus of elasticity of the same substance, I find the modulus of torsion to be $\frac{1}{11}$th of the modulus of elasticity in metallic substances.

XIII. *On a Differential Barometer.* *By the late* William Hyde Wollaston, *M.D. F.R.S. Communicated by* Henry Warburton, *Esq. F.R.S.*

Read February 5, 1829.

THE instrument which I am about to describe, was originally contrived with a view to determine the force with which heated air ascends in various kinds of chimneys: but since the action of the instrument depends on its rendering extremely small differences of barometric pressure discernible and capable of measurement with considerable accuracy, it will probably be found applicable to a variety of other purposes of more extensive utility.

In many open fire-places, the slight force with which the smoke ascends, is manifest from the facility with which it is forced back by any puff of wind that the shutting of a door, or window, or other accidental circumstance may occasion: but in some, which are more judiciously or more fortunately constructed, the draft is so considerable as to require a considerable supply of air.

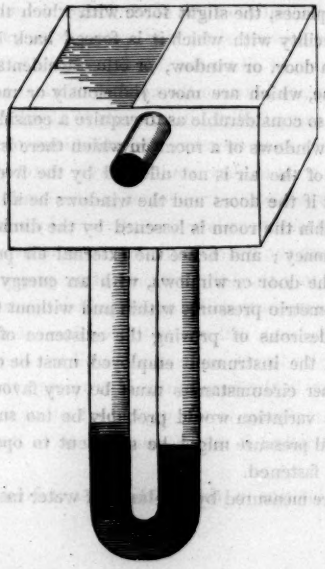
When the door or windows of a room, in which there is a fire, are open, the barometric pressure of the air is not affected by the free current of air that supplies the fire; but if the doors and the windows be all closed, then the barometric pressure within the room is lessened by the diminished weight of the heated air in the chimney; and hence the external air presses for admission at every crevice of the door or windows, with an energy proportioned to the difference of the barometric pressures within and without the chamber.

If any one were desirous of proving the existence of a difference by a mercurial barometer, the instrument employed must be of the very best construction, and all other circumstances must be very favourable to the experiment; otherwise the variation would probably be too small to be perceived, although the external pressure might be sufficient to open the door, if only closed without being fastened.

If the pressure were measured by a column of water instead of mercury, the

variations would of course be more perceptible; but the instrument, from its length, would be exceedingly incommodious. The corresponding advantage and disadvantage would be proportionally greater in employing a column of alcohol; and by having recourse to ether, we should arrive at the limit of inconvenience, as of sensibility, in any simple column.

The instrument which I have employed for this purpose is, on the contrary, very compact in its form, and the principle on which it is constructed is such, that any assignable degree of sensibility may be given to it. It consists of a tube of glass, having its internal diameter at least a quarter of an inch, bent in the middle into the form of an inverted siphon, with the legs parallel to each other. The extremities of the legs are cemented into the bottom of separate, but equal, cisterns, about two inches in diameter; one of these cisterns being closed on all sides, excepting by a small horizontal pipe opening laterally from its upper part; while the other cistern remains open.



Into a vessel so constructed, a small quantity of water is first poured, so as to occupy two or three inches of the lower part of the glass tube. Equal measures of oil are then poured into each cistern, so as to fill the upper part of both legs of the tube, and also to rise in each cistern to the depth of about half an inch.

When the two surfaces of the water in the two legs are seen to be on the same level, or have been rendered so, by equalizing the pressures of the incumbent columns of oil, the instrument is adjusted ready for use.

If the horizontal pipe from the closed cistern be now applied to the key-hole of a door or to any other perforation, through which air may enter by excess of external pressure, the pressure applied to the surface of the oil in that cistern will lower the water in the corresponding leg, and raise it in the opposite one, until the excess of the weight thus elevated is sufficient to balance the force by which the pressure of the external air exceeds that within the chamber.

It is not, however, the entire excess of the longer column of water which in this case acts as an equipoise; since that excess is counteracted by an equal elongation of the column of oil on the side depressed; so that the pressure exerted is only the difference between the column of water and an equal column of oil; which, in the case of olive oil, amounts to about $\frac{1}{11}$th part of the apparent elevation. In this case therefore the variations of this instrument are about eleven times as great as they would be, were water alone employed.

If for any other purpose an instrument of greater sensibility be required, the scale of its variations may be enlarged at pleasure, by mixing a greater or less quantity of alcohol with the water, until the excess of its specific gravity above that of oil is reduced to $\frac{1}{20}$th, $\frac{1}{30}$th, or in any other proportion; so that finally the spirit being of the strength called proof (which appears originally to have been named from this test), will rest with steadiness in no position, or being still further attenuated, will rise and suffer the oil to subside in the tube.

By a slight variation in the form of this instrument; that is by closing both the cisterns, and by applying to the upper part of each a trumpet-mouthed aperture, opening laterally; it may be made to serve the purpose of an Anemometer.

Captain Flinders informs us that on the coast of New Holland during the

prevalence of a sea-breeze, he had observed the barometer to be in general perceptibly higher than when a land-wind prevails; and he endeavours ingeniously to account for this appearance by the accumulation of air which takes place in front of any obstacle opposed to the air's motion, and which, therefore, occasions in it a greater barometric density.

It was on this principle that a species of wind-gauge was constructed by Dr. Lind, consisting of an inverted siphon, having the extremities of its two legs bent horizontally and in opposite directions. When the siphon is partially filled with water, and one of its horizontal extremities is exposed to a current of air, its pressure occasions the water on this side to descend, until its force is counteracted by the greater height of water in the opposite leg, the difference of the two columns being the measure of the force of the wind.

If a lighter fluid than water were employed in the construction of Dr. Lind's instrument, it would be rendered proportionally more sensible; but to such means of improvement there is a natural limit, since the scale could not, by means of any known fluid, be increased in a greater ratio than that of 4 to 5. Whereas by means of the instrument which is here proposed, the range of the index may be increased in any desired proportion, so as to measure the force of the gentlest flow of air.

XIV. *Some observations relating to the function of digestion.* By A. P. W. Philip, *M.D. F.R.S. L. & E.*

Read January 16, 1829.

No arguments are necessary to convince us of the importance of that function on which all parts of our frame depend for their nourishment. In one respect its organs may be regarded as of greater importance than even those which are more immediately essential to life. The sympathies of the stomach and first intestine are both more powerful and more extensive than those of any other part, and consequently more generally and in a greater number of ways contribute to the cause, and influence the course of all our more serious diseases.

I am induced to trouble the Society with the following observations, in the hope that I shall be able to place before them some points relating to the function of the stomach in a clearer point of view than has hitherto been done. In former papers which the Society have done me the honour to publish, and more fully in a Treatise on the Vital Functions, I have endeavoured by experiment to trace the different steps of the process of digestion in the stomach. It appeared that the food remains in a quiescent state, except that the part of it which lies next the stomach, as soon as it has undergone the effect of the gastric juice, is, in consequence of food thus prepared exciting a peculiar action in the muscular fibres of this organ, carried on towards the pylorus; through which it is propelled into the intestine, the next portion of food thus brought into contact with the stomach undergoing the same process; and so on, till the whole is in a state proper for that part of the digestive process which belongs to the first intestine.

Thus the muscular fibres of the stomach are in continual action during its function; for the gastric juice pervading the contents of the stomach to a certain extent, the change effected by it on each particular portion of the food is

nearly completed before the food is actually in contact with the stomach, as may be seen by inspecting that of an animal killed a few hours after a meal; and consequently is not detained when in contact with it. There is therefore a continual motion of the food in contact with its surface towards the pylorus, and the less digested part is continually approaching its surface.

It follows then that a failure of the function of the stomach may arise either from a proper gastric juice not being supplied, or the muscular power of the stomach failing to carry onward the digested part, and thus regularly to present to the stomach a new surface of food to be acted upon by that juice. It further appeared, that for the first of these purposes the power of the nervous system is necessary, the secretion of gastric juice failing as soon as the stomach is deprived of any considerable part of this power; but that the nervous power is not necessary for the other, the muscular power of the stomach still carrying on towards the pylorus any digested food which happens to be in it, or any food which had been acted upon by gastric juice which happened to be in it at the time, however much its nervous power be impaired; and this office is, as far as we can see, as readily performed as when the nervous power of the organ is entire.

The muscular fibres of the stomach therefore are stimulated by its contents, in the same way as those of the heart by the blood, the usual action of both being wholly independent of the nervous system, an inference confirmed by many other experiments beside those here referred to.

I have, as appears from the papers which the Society have done me the honour to publish, attempted to go a step further, and to show experimentally that the office of the nervous power in preparing the gastric juice, may be correctly imitated by exposing the living stomach to the influence of a voltaic pile after the supply of nervous power is interrupted. Those who were at first inclined to doubt this fact, have since publicly acknowledged, on witnessing the experiments, that the digestive process of the stomach supported by galvanism, is, as far as we can see, as perfect as that supported by the nervous power itself.

It is therefore evident that in the formation of the gastric juice, a chemical power can be substituted for that of the nervous system. I do not mean that, strictly speaking, its formation is to be regarded as a mere chemical process,

because it is only in a living stomach that galvanism can have such an effect; but this effect bears too strong an analogy to other chemical results to be wholly separated from them, although what we call life, whatever that may be, is necessary to its production.

The same effect, and one certainly of a very complicated nature, is here produced by the nervous power and a chemical agent; because when the latter is substituted for the former, the same effect takes place. It is a simple matter of fact. But it is maintained by some gentlemen that the same effect may be produced by a mechanical agent*. They have related several experiments which appeared to them to prove, that when after a part of the eighth pair of nerves is removed, and thus the due secretion of gastric juice prevented, it may be restored by mechanically irritating the cut ends of the lower portions of the divided nerves. If such be the fact, it must materially influence our views both with respect to the function of digestion and the other secreting processes of the animal body.

In judging of the result of such experiments, several things must be taken into the account which appear to have escaped the attention of those gentlemen.

At the time the animal is fed, in preparation for the experiment, there may be some food in the stomach, from previous meals, more or less digested, and there is always some gastric juice ready to act on any new food which may be presented to it. It is evident therefore, that although the secretion of gastric juice ceases at the moment of the excision of part of the eighth pair of nerves, some digested food must be found in the stomach for some hours after the operation; for, as I have ascertained by numerous trials, many hours are required in such experiments for the stomach to propel into the intestine the remains of food previously digested, or that digested by the gastric juice previously formed.

When therefore the contents of the stomach are examined in five or six hours, and generally even in ten or twelve, after the operation, more or less digested food is found lying next the surface of the stomach. But when the

* See a paper entitled, Mémoire sur le mode d'action des nerfs pneumogastriques dans la production des phenomènes de la digestion. Par MM. Breschet et Milne Edwards (lu à la Société Philomatique, le 19 Fevrier 1825).—(Extract des Archives generales de Médécine.)

animal survives the operation eighteen, twenty, or more hours, undigested food alone is found in it. The cause of so long a time being required wholly to expel the food, which has undergone any degree of the digestive process, appears to be, that as digested food alone excites that action of the stomach which propels it into the intestine, and the more perfectly it is digested, it excites this action the more readily, the last part of the digested food which has but imperfectly undergone the digestive process is expelled very slowly, so that it is very long before food wholly undigested alone is left.

That the longer the animal lives after the excision of part of the eighth pair of nerves, the less digested food is left in the stomach, is a fact now admitted by all who assisted at the experiments. Among the great number who have witnessed and been satisfied with their result, are Sir Humphry Davy, Mr. Thomas Andrew Knight, and Mr. Brodie, gentlemen whose experimental accuracy, in the opinion of the public, has never been surpassed.

Of this fact, the gentlemen to whose paper I have referred, are not aware. They maintain, indeed, that the only effect on the digestive process produced by the excision of part of the eighth pair of nerves, is, that it becomes more tedious, being as perfect as when the nerves are entire, if a sufficient length of time be afforded. In speaking of the animals in which part of the eighth pair of nerves has been cut out, and comparing them with the healthy animal, they say: "Enfin, si on laisse écouler un espace de temps plus grand encore entre l'operation et la mort des animaux, on pourra trouver que la digestion est completement achevée dans l'un comme dans l'autre cas."

It will easily be perceived to what errors, respecting the effect on digestion, of depriving the stomach of the office of the eighth pair of nerves, this misconception must lead. Its effect was increased in the experiments referred to, by the different animals in each experiment having been confined to the same quantity of food. The most hungry would of course digest it fastest and most perfectly. To judge fairly of the result of the experiment the different animals must be allowed equally to satisfy their appetite, to eat till, from their manner of eating, it is found that the appetite has equally abated in all.

Such are the circumstances which I conceive misled those gentlemen who maintain that they can produce a sensible effect on the contents of the stomach by any mechanical irritation of its nerves.

They also err in supposing that the muscular fibres of the stomach can be excited by irritating the eighth pair of nerves in the way that a muscle of voluntary motion may be excited through its nerves. The digested food is the natural stimulus of the muscular fibres of the stomach in its usual function, as the nervous power is of the muscles of voluntary motion in theirs; and we cannot through the nerves excite the former as we do the latter class of muscles. The muscular action of the stomach resembles that of other hollow muscles, in being excited by its contents.

The mechanical irritation employed by those gentlemen, in endeavouring to excite the digestive process after a portion of the eighth pair of nerves had been removed, was that of a thread attached to the cut extremities of the lower portions of the eighth pair of nerves and fastened to the neighbouring muscles, by which the motions of respiration kept the part in a state of constant irritation.

In my Treatise on the Vital Functions, a similar experiment is related, in which the cut extremity of the lower portions of the nerves was fastened to a thread tied round the neck of the animal, by which it was in like manner kept in a state of constant mechanical irritation; yet in the stomachs of the animals after they had lived more than twenty hours,—for the experiment was made more than once,—nothing but undigested food was found. This experiment, with some others connected with it, was made publicly in the rooms of the Royal Institution; and all who felt an interest in the subject admitted to see the results, nor was there one who expressed a doubt respecting them.

As, however, in the experiments just mentioned the position of the nerves was more disturbed, and the thread was not applied as in the experiments to which I have referred, Mr. Cutler, at my request, was so good as to make the following experiment.—Three rabbits, after a fast of the same duration, were fed in the same way. In two of them a portion of each of the eighth pair of nerves was removed. The third rabbit was left undisturbed. In one of those in which the portions of nerve were removed, the cut end of the lower part of the nerves was by means of a bit of thread fastened to the neighbouring muscles, as in the experiment referred to. This rabbit died in ten hours, at which time the others were killed in the usual way.

Mr. Cutler then took out the stomachs of all of them, slit them open, and laid them on the same plate; and Mr. Brodie was requested to examine and

give his opinion respecting their contents, without having been told which was which. He at once pointed out the healthy stomach, the whole contents of which had undergone the action of the gastric juice. After carefully examining, and with an instrument moving about the contents of the other stomachs, he declared he could discover no difference in them. Both stomachs were chiefly filled with undigested food, the animals not having lived long enough after the operation for the expulsion of some imperfectly digested food that still remained in both.

The foregoing experiments convinced those who witnessed their results, that the irritation caused by the attachment of the cut end of the nerves to the muscles, had no effect whatever in promoting the digestion of the food.

Were it possible, as in the case of the nerve of a muscle of voluntary motion, to excite the eighth pair to perform its office after its communication with the brain is wholly intercepted, it is surely impossible that this could go on for many hours, which are necessary for the digestion of the food. A nerve of voluntary motion, if kept in a state of excitement after its separation from the brain or spinal marrow, loses its power in a very short time, at most a few minutes.

The result of the foregoing experiment may be known before the death of the animals. It appears from what was said in other papers which I had the honour to lay before the Society, and which were published in the Philosophical Transactions, that the effect of the excision of part of the eighth pair of nerves on the lungs, as well as on the stomach, is obviated by galvanism, the animals (the dog and rabbit were those on which the experiments were made) breathing under its influence as freely as in health. It is clear, that if the power of the nerve be restored, its restoration must be as evident in the function of the one organ as the other, these nerves being equally essential to both. In the foregoing experiments both the animals were affected with extreme dyspnœa, the mechanical irritation of the nerves having no more effect in relieving this symptom, than in promoting the due action of the stomach.

XV. *Experiments on the friction and abrasion of the surfaces of solids.* By George Rennie, *Esq. F.R.S.*

Read June 12, 1828.

THE paper now offered to the consideration of the Royal Society, comprises the results of part of a series of experiments undertaken in the year 1825, with a view to determine the measure of the retardations of bodies in motion, when affected by the attrition of their surfaces, and by mediums of different densities.

From the attention that has hitherto been paid to this important branch of mechanical science, and from the many elaborate dissertations and experiments that have appeared at different periods, it would naturally be concluded, that the subject had been so fully elucidated, as to admit of little if any further investigation: but the diversity of opinions still prevalent among philosophers, and the difficulty of reducing to a satisfactory state the doctrines already advanced, incline me to the opinion that the subject is as yet but imperfectly understood. This may be attributed in a great degree to the very defective state of our knowledge of the properties of materials, and the difficulty or rather impossibility of subjecting them to geometrical mensuration. The science of mechanics considers forces as reduced to the simple questions of mathematical analysis, without regard to the properties of matter or the phænomena incident thereto: but in rendering forces sensible, we are necessarily compelled to make use of agents, or intermediate bodies termed machines, the employment of which in transmitting motion, in modifying its action, or in restoring the equilibrium between forces of different intensities, constitutes the object of every mechanical operation. The solution of this question therefore involves the conditions of equilibrium, both of simple and compound machines; the transmission of motion under different circumstances; the construction and combination of the different parts of machines, and the properties of the materials of which these parts are composed.

On a former occasion an attempt was made to develope some of the properties of solid bodies in resisting the action of a disruptive force*, the measure of which was represented by the sum and qualities of the particles displaced. The connection may be traced in the present inquiry, which relates principally to the resistance arising from the displacement, or rupture of the superficial asperities of bodies in motion when brought into contact by extreme pressure, and is analogous to the cohesive state of a body acted upon by opposite but contrary forces. But the cases investigated by experimentalists have seldom been carried to the extent necessary to produce a disrupture of the prominencies, being generally confined to the definition of friction as designated by writers on mechanics, to be the force expended in raising continually the surface of pressure by an oblique action; the surfaces being represented by a series of inclined planes acting against each other in alternate succession. The measure of friction therefore being supposed to depend upon the angles of the prominencies and the elementary structure of the bodies, the effect of polishing could only be to diminish those prominencies without altering their curvature or inflections. The expense of force therefore ought still to remain the same in both cases†. In this hypothesis it is reasonable to concur, experiment proving, that the amount of friction bears immediate reference to the elementary structure of bodies; and although the doctrine of inclined planes admits of a ready comprehension of the causes of this kind of resistance under certain circumstances, a very slight investigation of the nature of the bodies themselves will exhibit their asperities under every possible configuration. The amount of resistance will depend upon the degree of pressure, the approximation or rather the engagement of the asperities and concavities, and the nature of the surfaces of which fibrous, soft, or hard bodies are composed. To surmount, bend, or detach these asperities under the circumstances of pressure, area, and velocity, demands a proportionable exertion of force; and it is by the determination of this force under all cases, that we can alone arrive at an estimation of the performance of machines.

The nature of friction has excited the attention of most of the writers on mechanics, from the period of the first two dissertations of Amontons in the

* Experiments on the Strength of Materials:—Philosophical Transactions 1817.

† Leslie's Experimental Philosophy.

year 1699, down to the more elaborate researches of Coulomb and Vince in 1779 and 1784. Amontons was the first that attempted to develope and reduce theory to calculation. He affirmed that friction was not augmented by an increase of surface, but only by an increase of pressure*; and in a subsequent paper, illustrated by some experiments on wood and metals pressed by springs of known intensity, he drew similar conclusions, with the addition that friction was $\frac{1}{3}$rd of the pressure, and that the amount was the same both with wood and metals when unguents were interposed. He likewise concluded, that friction increased or diminished with the velocity, and varied in the ratio of the weight and pressure of the rubbing parts, and the times and velocities of their motions. These hypotheses were adopted more or less by most of the philosophers after Amontons, but particularly by De la Hire†, who satisfied himself by several experiments of the truth of Amontons' conclusions; but they were questioned by Lambert, although without the test of experiment. Parent suggested an investigation of the subject in his proposition of the Spheres, and by determining the angle of equilibrium, at which a body resting on an inclined plane commenced sliding. And the celebrated Euler, in a very elaborate paper‡, conceived it to depend upon the greater or less approximation of the asperities of the surfaces brought into contact by pressure, the resistance to which he allows to be $\frac{1}{3}$rd of the pressure; the same as Amontons. Of the effect of velocities he was however uncertain; but observed that when a body begins to descend an inclined plane, the friction of the body will be to its weight or pressure upon the plane, as the sine of the plane's elevation to its cosine, &c. But when the body is in motion, the friction is diminished one half. Muschenbroek and others maintained that friction increased with the surface; and Bossut distinguished it into two kinds; the first being generated by the gliding, and the second by the rolling of the surface of a body over another: and remarked, that it was affected by time, but that it neither followed the ratio of the pressure nor the mass. Brisson§ attempted to construct a table of coefficients, to denote the value of the friction of different substances; but they are inapplicable to practical purposes, for want of proper experiments. Desaguliers considered the nature of friction with a good deal of attention,

* Sur la Force des Hommes et des Chevaux, et de la Resistance causé dans les Machines.

† Mémoires de l'Academie des Sciences. ‡ Ibid.

§ Brisson, Traité de Physique.

but principally with reference to the rigidity of cords. He however quotes the experiments of Camus as best calculated to illustrate the subject; nevertheless they were made on too small a scale to derive any satisfactory conclusions. Schober and Meister coincided with Muschenbroek in the opinion, that the spaces were as the squares of the times in the case of a body uniformly accelerated. The opinions of many other eminent philosophers, such as Leibnitz, Varignon, Leupold, Bulfinger, Daniel Bernoulli, Ferguson, Rondelet, Gregory, Leslie, Young, Olivier*, &c. might be quoted. But it is to Coulomb principally that we are indebted for the knowledge we possess of this kind of resistance.

In the year 1779 the Academy of Sciences at Paris, being desirous of rendering the laws of friction, and the effects resulting from the rigidity of cords applicable to machines,—Coulomb undertook in the arsenal at Rochfort a very extensive series of experiments, which he afterwards published in 1781 under the title of "Théorié des Machines simples, en ayant égard au Frottement de leurs Parties, et à la Roideur des Cordages†." The memoir is divided into two parts. The first treats of the friction of surfaces gliding over each other, and the second enters into an examination of the rigidity of cords, and the friction of the rotary movements of axles. Coulomb commences his work by examining the friction of plane surfaces gliding over each other, distinguishing it into two kinds; the first resulting from time, and the second from velocity. The first may depend on four different causes, viz.

1st. The nature of the bodies in contact.

2nd. The extent of surface.

3rd. The pressure on the surface.

4th. The time the surfaces have been in contact. And he even adds a

5th. The state of the atmosphere; which he however thinks may have little influence.

The case of bodies gliding over each other with a certain velocity he considered to be referable to the first three causes, besides the velocity of the planes in contact.

With regard to the physical cause of friction, he coincides with the opinions of Amontons and others, that it arises from the entangling of the asperities,

* Sur les diverses Espèces de Frottements, &c. (not published.)

† Mémoires des Sçavans Etrangers, tome 163 & 333.

which can only be disengaged by bending or breaking. These experiments led to some important results, viz.

1st. That the friction of wood on wood without unguents was in proportion to the pressure which attained its maximum in a few minutes after repose.

2nd. That the effects of velocities were similar; but the intensities were much less to keep the body in motion, than to detach it from a state of rest, oftentimes in the ratio of 22 : 95.

3rd. That in the case of the metals the results were likewise similar; but the intensity was the same whether to disturb or maintain the motion of the body.

4th. That with heterogeneous surfaces, such as those of woods and metals gliding over each, the intensity did not attain its limit sometimes for days.

In general, however, with woods and metals without unguents, velocities were found to have very little influence in augmenting friction, except under peculiar circumstances.

The treatise of COULOMB is illustrated by a great variety of interesting experiments, and forms the most valuable work we possess on the subject.

In the year 1784, Dr. VINCE endeavoured by some very ingenious experiments to determine the law of retardation together with the quantity, and the effect of surface on friction. The results were, that the friction of hard bodies in motion was an uniformly retarding force, but not so with cloth and woollen, which were found in all cases to produce an increase of retardation with an increase of velocity.

That the quantity of friction amounted to about $\frac{1}{4}$th of the pressure, and that it increased in a less ratio than the quantity of matter or weight of the body.

That when the surfaces varied from 1.61 : 1 to 10.06 : 1, the smallest surface gave the least friction: and finally, that friction was greatly influenced by cohesion.

Dr. VINCE's conclusions regarding the laws of retardation were partly confirmed by the late ingenious Mr. SOUTHERN of Soho, who in a letter to Dr. VINCE in 1801, communicated the results of several experiments on the surfaces of the spindles of grindstones moving with great velocities; when it was found that with the rubbing surfaces moving at the rate of 4 feet per second over a length of surface of 1000 feet, the resistance arising from the friction of 3700lbs. of matter, only amounted to $\frac{1}{10}$th of the weight.

In the year 1786 and subsequently, the late Mr. RENNIE made several ex-

periments on the friction and resistance of heavy machinery. The results varied under different circumstances; but it appeared that an augmentation of resistance took place in proportion to the quantity of machinery put into action. In one instance in the ratio of 1 to 5, when it absorbed from one-fifth to one-tenth of the power expended.

This anomaly, as compared with the ratio of surfaces in the present experiments, can only be accounted for, from the irregularity of the movements and the difficulty of producing simultaneous actions in complicated machinery; the more especially as the results were affected by contingencies which could not be properly estimated; some of the elements on which the deduction is founded not being stated. The resistance was likewise increased by reversing the direction of motion. The velocities being very moderate, and hardly exceeding 120 feet a minute, appeared to have had no influence: but the experiments related principally to the resistances produced by different kinds of machinery. The experiments of M. Boistard* on the gliding of stones with a view to develope the equilibrium of arches, led him to conclude that the relation of the friction to the pressure was constant; that asperity of surface did not alter its value, which generally amounted to $\frac{1}{3}$ths of the pressure.

From similar experiments M. Rondelet concluded†,

1st. That the rougher the surface of stones, the greater the power required to move them.

2nd. That the greater the insistent weight, the greater the resistance: but as the inequalities are apt to be broken, the maximum force required to over come the friction ought to be equal to produce that effect, whatever be the weight of the stone.

3rd. That this force ought rather to be in the ratio of the hardness of the stone than of its weight.

4th. The amount of friction varied from one-half to one-third of the insistent weight.

5th. The angle of equilibrium of similar stones was about 30 degrees. And

6th. Finally, extent of surface did not alter its value.

The experiments of Morisot on the grinding and polishing of stones, and of Maniel and Pasley on the pressure and equilibrium of earths, present

* Recueil d'Experiences et d'Observations, &c. sur le Pont de Nemours.

† L'Art de Batir. Tome iii. 1808.

some interesting results; but it is only recently that our knowledge of the subject has been materially enlarged.

The agitation of the canal and rail-road question in the years 1824 and 1825, and the invention or rather revival of a mode of applying steam in lieu of animals to carriages on rail-roads, led to the most extravagant conclusions: and although the doctrines of COULOMB and VINCE, relative to the equality of resistances under different velocities, have been still further confirmed by the experiments of many able persons in this country, such as CHAPMAN, GRIMSHAW, WOOD, TREDGOLD, PALMER, ROBERTS, and others, and much valuable information elicited;—our progress in the science has been but slow and unsatisfactory. Sensible of these defects, and being unable to profit by the valuable treatises subsequently published, it occurred to me that a series of experiments founded on the omissions of former writers would be extremely desirable.

The present series of experiments relates to the friction of attrition. This branch of the science comprehends the resistance occasioned by solid bodies,—such as ice, cloth, paper, leather, wood, stones, metals, &c. gliding over each other simply, or by the intervention of semi-fluids or unguents, such as oil, tallow, &c.

The object has likewise been to determine the powers to resist abrasion under the circumstances of surface, pressure, and velocity. Examples have been sought,

1st. From ice, by the resistance of its surface to sledges, skates, &c.

2nd. From cloth, by its remarkable properties of resistance in opposition to the law observed by solids.

3rd. From leather, by its great utility in the pistons of pumps, &c.

4th. From wood, in its application to pile driving, carpentry, launching of ships, &c.

5th. From stones, as relating to the equilibrium of arches and buildings. And

6th. From metals, from their universal application to machinery; but more particularly to wheel carriages and rail and other roads, on which a great many experiments have been made.

Experiments on a great scale, however, frequently involve so many contradictions, from the difficulty of obtaining the necessary elements, that I have deemed it preferable to offer the present series, as comprehending in a greater degree most of the cases in question, and affording a more systematic view of the nature of the investigation.

The apparatus employed in performing the experiments on the friction of attrition, Plate IV, consists simply of a strong table accurately made and adjusted, and provided with a platform capable of being elevated to any angle within thirty degrees, as shown by the graduated arc. The substances tried were placed on the platform and in the sliding block above, to which the scale and weights for bringing the substances into closer contact were suspended: a cord going over a pulley was attached to the sliding block, which received its motion from weights put into the moving scale. The different phænomena were then accurately recorded, as appears by the accompanying Tables, and the conclusions derived from them.

Table I.

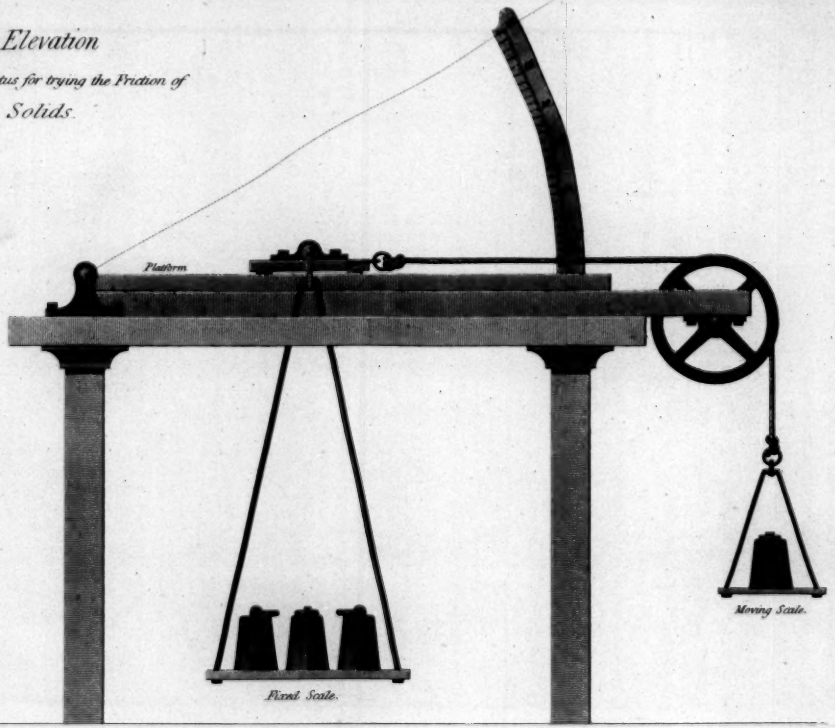
Experiments on the Friction of 3 square inches surface with Cloth.

Weight on surface.	Weight required to move it.	Proportion.	Weight on surface.	Weight required to move it.	Proportion.
Black Single Kerseymere. No. 1.			Superfine Blue. No. 2.		
lbs.	lbs. oz.		lbs.	lbs. oz.	
1	1 6		1	1 3	
2	2 4		2	2 12	
5	4 2	1 21	5	5 3	
10	6 4	1 60	10	8 4	1 21
20	9 13	2 03	20	12 11	1 57
28	13 2	2 13	28	15 5	1 82
56	20 11	2 70	56	22 11	2 47
Drab Milled Kerseymere. No. 3.			Drab Kersey Hunter. No. 4.		
lbs.	lbs. oz.		lbs.	lbs. oz.	
1	1 11		1	1 5	
2	2 11		2	1 15	1 03
5	5 3		5	3 8	1 43
10	took 2½ lbs. in addition to start it } 7 13	1 28	10	5 4	1 90
10	after remains 10 lbs. it took to start it } 12 10		20	8 11	2 30
20	12 11	1 57	28	10 0	2 80
28	16 7	1 70	56	19 3	2 92
56	25 3	2 22			
Strong Drab. No. 5.					
lbs.	lbs. oz.				
1	0 15	1 06			
2	1 8	1 33			
5	3 2	1 60			
10	4 11	2 13			
20	7 11	2 60			
28	9 12	2 87			
56	17 14	3 13			

Elevation

of the Apparatus for trying the Friction of

Solids.



Remarks.

1. That with fibrous substances, such as cloth, friction diminishes with an increase of weight.
2. That friction is greater (cæteris paribus) with fine cloths than with coarse cloths.
3. That friction is greatly increased by time.
4. That friction varies from one-third to an amount greater than the total weight.

TABLE II. Experiments on the Velocities with Drab Milled Kerseymere, No. 3.

Weight on surface.	Weight required to move it.	Total space passed over.	Time in seconds.		Remarks.
Of 9 square inches surface.					
lbs.	lbs. oz.	inches.			
1	1 8	24		45	From 1lb. to 2lbs. the adhesion is greater than the weight on surface.
				38	
				30	
				22	
				23	Velocities very irregular.
1	1 5			24	
				25	
				40	
				37	Velocities very irregular.
2	2 5			31	
		half way in 17 sec. the whole in		26	
			17	27	* Denote the experiments that approximate the nearest to an uniform velocity.
2	2 5		21	30*	
			33	53	
			17	30*	Results very irregular, owing perhaps to the fibres of the cloth having been previously compressed.
5	4 3		29	45	
10	6 7	mean of 3 trials	45	63	
20	9 7		30	50	
Of 18 square inches surface.					
lbs.	lbs. oz.	inches.	1st half.	2nd half.	Increase of surface shows an increase of resistance with equal weights of 20lbs. Time nearly doubles the resistance.
20	13 6	mean of 3 trials 21	22	33	
20	after remains 14 hrs. it took to start it 23 3				
Of 27 square inches surface.					
lbs.	lbs. oz.	inches.	1st half.	2nd half.	Three times the surface nearly three times the resistance.— Velocities irregular. Vide Vince's Experiments.
1	2 8	mean of 3 trials 18	4	14	
2	3 10		30	73	
5	6 7		25	60	
10	10 2		28	55	Nearly uniform.

1. From the foregoing experiments it appears that velocities observe no particular law, except in three instances, where the last halves of the space passed over approximate to the first halves.
2. That increase of surface very much increases the resistance.

TABLE III. On the Friction with Cloth at different angles of elevation.

Weight on surface.	Moved at degrees.	Space passed over.	Time in seconds.	Proportion.	Weight on surface.	Moved at degrees.	Space passed over.	Time in seconds.	Proportion.
Of 3 square inches.					Of 27 square inches.				
lbs.	°	inches.			lbs. oz.	°	inches.		
10	37.00	24	55	1.327	13 8	45.00	18	32	1.000
20	28.20		55	1.855	20 0	40.30		42	1.171
28	26.00		47	2.051	28 0	35.45		32	1.389
56	20.45		44	2.640	56 0	26.00		28	2.052

1. In comparing the results given by the angles of repose with the results given by the horizontal surfaces on similar kinds of cloth, there is a slight variation.

2. The second series of experiments afford no measure of comparison, from the inadequacy of the weights of 10lbs. being unable to give motion to the upper surface, 13lbs. 8oz. gives an approximation.

3. The less the weight, the greater the angle of repose.

4. Increase of surface produces a very great increase in the angles of repose.
The times very variable, diminish with increase of weight.

5. Velocities likewise variable.

TABLE IV. On the Friction of different Woods two square inches surface.

Weight on surface.	Weight required to move it.	Proportion.	Weight per square inch.	Average.	Weight on surface.	Weight required to move it.	Proportion.	Weight per square inch.	Average.
Red Teak on Red Teak.					American live Oak on American live Oak.				
cwt.	lbs. oz.		cwt. qrs.		cwt.	lbs. oz.		cwt. qrs.	
½	6 14	8.14	0 1		½	7 15	7.05	0 1	
1	14 2	7.92	0 2		1	14 13	7.56	0 2	
2	23 3	9.66	1 0		2	25 15	8.63	1 0	
3	38 1	8.82	1 2		3	36 11	9.15	1 2	
4	52 3	8.58	2 0		4	55 11	8.04	2 0	
5	64 2	8.73	2 2		5	70 3	7.97	2 2	
6	71 12	9.36	3 0	8.82	6	86 3	7.79	3 0	7.65
7	84 3	9.31	3 2		7	109 7	7.16	3 2	
8	90 8	9.90	4 0		8	128 4	6.98	4 0	
9	120 11	8.35	4 2		9	140 3	7.19	4 2	
10	126 5	8.86	5 0		10	154 1	7.26	5 0	
11	141 15	8.67	5 2		11	162 14	7.56	5 2	
12	154 3	8.71	6 0		12	187 5	7.17	6 0	
13	170 10	8.53	6 2						
Pine on Pine.									
cwt.	lbs. oz.		cwt. qrs.						
¼	16 3	3.33	0 1						
½	27 14	4.01	0 2	3.40					
2	68 4	3.27	1 0						
3	111 5	3.01	1 2						

Experiments on the Friction of different Woods two square inches surface.

Weight on surface.	Weight required to move it.	Proportion.	Weight per square inch.	Average.
Black Beech on Black Beech.				
cwt.	lbs. oz.		cwt. qrs.	
½	8 6	6.68	0 1	
1	15 5	7.31	0 2	
2	28 0	8.00	1 0	
3	45 3	7.43	1 2	
4	69 7	6.45	2 0	
5	83 3	6.73	2 2	7.13
6	100 4	6.70	3 0	
7	115 11	6.77	3 2	
8	124 10	7.18	4 0	
9	132 3	7.62	4 2	
10	148 11	7.53	5 0	
English Oak on English Oak.				
cwt.	lbs. oz.		cwt. qrs.	
½	7 0	8.00	0 1	
1	15 0	7.46	0 2	
2	29 3	7.67	1 0	
3	43 2	7.79	1 2	7.83
4	55 0	8.14	2 0	
5	70 3	7.97	2 2	
Elm on Elm.				
cwt.	lbs. oz.		cwt. qrs.	
½	10 0	5.60	0 1	
1	22 1	5.07	0 2	
2	35 5	6.34	1 0	
3	53 2	6.32	1 2	
4	72 3	6.20	2 0	5.86
5	87 11	6.38	2 2	
6	108 4	6.20	3 0	
7	145 3	5.39	3 2	
8	168 11	5.31	4 0	
Yellow Deal on Yellow Deal.				
cwt.	lbs. oz.		cwt. qrs.	
½	19 7	2.88	0 1	
1	37 9	2.98	0 2	
2	76 3	2.94	1 0	2.88
3	113 0	2.97	1 2	
4	147 13	3.03	2 0	
5	224 0	2.50	2 2	

Weight on surface.	Weight required to move it.	Proportion.	Weight per square inch.	Average.
Norway Oak on Norway Oak.				
cwt.	lbs. oz.		cwt. qrs.	
½	8 3	6.83	0 1	
1	14 5	7.82	0 2	
2	26 4	8.53	1 0	
3	41 3	8.17	1 2	
4	56 7	7.93	2 0	7.67
5	67 3	8.33	2 2	
6	80 4	8.37	3 0	
7	102 0	7.68	3 2	
8	164 3	5.45	4 0	
Hornbeam on Hornbeam.				
cwt.	lbs. oz.		cwt. qrs.	
½	8 10	6.49	0 1	
1	16 3	6.91	0 2	
2	30 5	7.38	1 0	
3	46 11	7.19	1 2	6.57
4	65 5	6.85	2 0	
5	83 1	6.74	2 2	
6	105 2	6.39	3 0	
7	167 3	4.68	3 2	
Honduras Mahogany on Honduras Mahogany.				
cwt.	lbs. oz.		cwt. qrs.	
½	12 7	4.50	0 1	
1	26 0	4.30	0 2	
2	39 3	5.71	1 0	
3	59 5	5.66	1 2	
4	74 7	6.01	2 0	
5	92 3	6.07	2 2	5.96
6	107 6	6.25	3 0	
7	118 2	6.63	3 2	
8	136 4	6.57	4 0	
9	154 1	6.54	4 2	
10	171 0	6.54	5 0	
11	182 3	6.76	5 2	
White Deal on White Deal.				
cwt.	lbs. oz.		cwt. qrs.	
½	18 12	2.98	0 1	
1	29 5	3.82	0 2	3.81
2	48 3	4.94	1 0	

Table V.

Experiments on the Friction of two square inches surface of Wood at different angles of elevation.

Weight on surface.	Moved at degrees.	Time in descending 11 inches.	Proportion.
Red Teak on Red Teak.			
lbs.	° ′	sec.	
10	8 00	18	7.116
20	7 45	15	7.348
28	7 15	20	7.861
56	7 00	16	8.144
Black Beech on Black Beech.			
lbs.	° ′	sec.	
10	8 15	20	6.897
20	7 20	17	7.770
28	7 40	19	7.429
56	6 40	21	8.556
English Oak on English Oak.			
lbs.	° ′	sec.	
10	9 30	17	5.976
20	8 30	17	6.691
28	7 40	18	7.429
56	7 30	20	7.596
Hornbeam on Hornbeam.			
lbs.	° ′	sec.	
10	10 00	20	5.671
20	9 15	21	6.140
28	8 30	20	6.691
56	8 15	19	6.897
Yellow Deal on Yellow Deal.			
lbs.	° ′	sec.	
10	15 00	10	3.732
20	17 00	9	3.271
Pine on Pine.			
lbs.	° ′	sec.	
10	16 00	14	3.488
20	17 00	11	3.271

Weight on surface.	Moved at degrees.	Time in descending 11 inches.	Proportion.
American live Oak on Red Teak.			
lbs.	° ′	sec.	
10	9 00	22	6.314
20	8 00	24	7.116
28	8 30	20	6.691
56	7 45	25	7.348
Norway Oak on Norway Oak.			
lbs.	° ′	sec.	
10	8 00	19	7.116
20	7 30	20	7.596
28	7 00	20	8.144
56	6 20	25	9.010
Elm on Elm.			
lbs.	° ′	sec.	
10	11 40	19	4.843
20	10 30	18	5.396
28	10 00	19	5.671
56	9 30	19	5.976
Honduras Mahogany on Hornbeam.			
lbs.	° ′	sec.	
10	12 00	22	4.705
20	12 30	21	4.511
28	11 45	21	4.808
56	11 20	23	4.990
White Deal on White Deal.			
lbs.	° ′	sec.	
10	18 00	10	3.078
20	12 30	11	4.511

REMARKS.—From the foregoing experiments it appears that there is a great deal of irregularity in the results.

Increase of pressure scarcely increasing the resistance. This may arise in some degree from the surfaces becoming condensed, and thus rendered less liable to abrasion. In some of the cases abrasion had already commenced, but it was not convenient to pursue the experiment further.

The soft woods present more resistance than the hard woods.

Yellow deal on yellow deal being the greatest.

Red teak on red teak the least.

According to Mr. KNOWLES of the Navy Office, F.R.S., the weight of the Prince Regent of 120 guns on the slips previous to launching, was 2400 tons; which, divided by the area of the sliding surface of her bilge-ways (equal to 149,184 square inches), gives a pressure of 36lbs. per square inch.

But the weight of the Salisbury of 58 guns on the slips, according to the area of her bilge-ways, was 44lbs. per square inch. Now, by the foregoing Table, the average force required to put in motion the three different kinds of oak, under a pressure of 56lbs. per inch, is about $\frac{1}{4}$th of the pressure, which proportion prevails even as high as 6cwt. per inch area: and by Table IX. we find that soft soap (the ingredient mostly used for diminishing the friction of bilge-ways under a pressure of 56lbs per inch,) gives about $\frac{1}{12}$th of the pressure for the friction. Hence the angle at which a building slip should be laid can be easily determined. COULOMB even makes 49lbs. per square inch, and $\frac{1}{17}$th for the pressure for hogslard.

The weight of the middle arch (of 151 feet 9 inches span) of the New London Bridge, together with the centres, is 4900 tons. This acting upon the surface of the striking wedges equal to 540 square feet, gives a pressure of 140lbs. per square inch. The angles of inclination of the wedges are equal to 8° 45′, and their surfaces are covered with sheets of copper well coated with tallow. On removing the check pieces, the wedges commenced gliding back slowly and uniformly by the gravity of the arch and centres, and the motion was checked and continued until the arch was left in equilibrio.

PLATE V.

This apparatus was constructed both for brass and iron. The pivots were accurately turned, and the suspending slings loosely hung. The total space passed over did not exceed four inches and a half. The cord was of the best sash-line, and the pulley very sensible. The rigidity of the former and friction of the latter were accurately ascertained, by trials at different weights. The block was of cast iron accurately bored. The axle was allowed to have full play in the block, in order that no binding might take place. The space passed through was denoted by marks on the axle and block. The time by a seconds watch.

An improvement was afterwards made in the apparatus, by substituting a roller of cast iron working in a block, and having a cord wound round its surface so as to allow of a descent of the moveable weight of 21 feet.

Table VI.

Experiments on extent of surface with Metals.

Wt. to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.	Weight required to move it.	Proportion.	Weight to 1 inch of area.	Wt. to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
	Laid flat.			Laid edgewise.				Laid flat.			Laid edgewise.		
	Cast Iron on Cast Iron.							Hard Brass on Cast Iron.					
	Area of surface 44 inches.			Area 6¾ inches.				Area of surface 48 inches.			Area 7¾ inches.		
lbs.	lbs. oz.		lbs. oz.	lbs. oz.		lbs. oz.	lbs.	lbs. oz.		lbs. oz.	lbs. oz.		lbs. oz.
14	2 2	6.58	0 5.09	2 4	6.20	2 1.1	14	1 14	7.4	0 4¼	1 11	8.3	1 12
24	3 3	7.53	0 8.72	3 11	6.50	3 8.8	24	3 5	7.2	0 8	4 0	6.0	3 1
36	4 14	7.38	0 13.10	5 14	6.12	5 5.3	36	4 9	7.8	0 12	6 0	6.0	4 10
48	6 8	7.38	1 1.40	7 10	6.30	7 1.7	48	6 4	7.6	1 0	7 13	6.1	6 3
60	8 4	7.27	1 5.80	9 8	6.30	8 14.2	60	7 12	7.7	1 4	9 0	6.6	7 11
72	10 0	7.20	1 10.20	11 7	6.29	10 10.6	72	9 12	7.3	1 8	11 0	6.5	9 4
84	11 10	7.23	1 14.50	13 5	6.31	12 7.1	84	11 8	7.3	1 12	13 2	6.4	10 13
96	13 12	6.98	2 2.90	15 5	6.27	14 3.5	96	13 1	7.3	2 0	14 8	6.6	12 6
	Yellow Brass on Cast Iron.							Tin on Cast Iron.					
	Area of surface 44 inches.			Area 6¾ inches.				Area of surface 44 inches.			Area 6¾ inches.		
lbs.	lbs. oz.		lbs. oz.	lbs. oz.		lbs. oz.	lbs.	lbs. oz.		lbs. oz.	lbs. oz.		lbs. oz.
14	1 15	7.22	0 5.09	2 1	6.79	2 1.1	14	2 8	5.60	0 5.1	2 12	5.09	2 1.1
24	3 7	6.98	0 8.72	3 8	6.85	3 8.8	24	4 7	5.40	0 8.7	4 8	5.33	3 8.8
36	5 6	6.70	0 13.10	5 1	7.11	5 5.3	36	6 0	6.00	0 13.1	6 7	5.59	5 5.3
48	7 3	6.67	1 1.40	6 10	7.24	7 1.7	48	8 7	5.68	1 1.4	8 14	5.40	7 1.7
60	9 3	6.53	1 5.80	9 3	6.53	8 14.2	60	9 13	6.11	1 5.8	9 13	6.11	8 14.2
72	11 5	6.36	1 10.20	10 5	6.98	10 10.6	72	12 5	5.84	1 10.2	11 13	6.09	10 10.6
84	13 5	6.30	1 14.50	13 12	6.10	12 7.1	84	14 5	5.86	1 14.5	14 5	5.86	12 7.1
96	15 13	6.07	2 2.90	15 1	6.37	14 3.5	96	16 4	5.90	2 2.9	16 4	5.09	14 3.5

From the foregoing experiments it appears that the friction of

Cast iron upon cast iron laid flat, varies . . . . . from 6.58 to 7.53

Cast iron upon cast iron laid edgewise, varies . . . . . from 6.2 to 6.5

Of hard brass upon cast iron laid flat, varies . . . . . from 7.2 to 7.8

Of hard brass upon cast iron laid edgewise, varies . . . . from 6.0 to 8.0

Of yellow brass upon cast iron laid flat, varies . . . . . from 6.09 to 7.22

Of yellow brass upon cast iron laid edgewise, varies . . . from 6.1 to 7.24

Of tin upon cast iron laid flat, varies . . . . . from 5.4 to 6.11

Of tin upon cast iron laid edgewise, varies . . . . . from 5.09 to 6.11

That the friction is nearly the same with cast iron and brass whether the load be applied on the broad side or on the narrow side of the plates, although the areas of the surfaces are to each other as 6.22 : 1.

That tin being a softer metal and more easily abraded: the friction increases when a load is applied above 8lbs. per square inch, but remains nearly the same with the broad side as with the narrow side. Generally speaking, the friction is less with the broad side than with the narrow side.

Table VII.

Experiments on the Friction of different Metals with the weights increased from 14lbs. to 192lbs.

Weight to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
Brass on Wrought Iron. Length 6¼ inches, Width ⅞. Area 5.906.			
lbs.	lbs. oz.		lbs. oz.
14	2 2	6.58	2 5.9
24	3 11	6.50	4 1.0
36	4 14	7.38	6 1.5
48	6 6	7.52	8 2.0
60	8 0	7.50	10 2.5
72	9 6	7.68	12 3.0
84	10 10	7.90	14 3.5
96	12 9	7.64	16 4.0
192	27 0	7.11	32 8.0
Soft Steel on Wrought Iron. Area 5.906.			
lbs.	lbs. oz.		lbs. oz.
14	2 8	5.60	2 5.9
24	4 8	5.33	4 1.0
36	6 13	5.28	6 1.5
48	9 5	5.15	8 2.0
60	12 6	4.84	10 2.5
72	14 13	4.86	12 3.0
84	17 5	4.85	14 3.5
96	19 4	4.98	16 4.0
192	32 8	5.90	32 8.0
Brass sliding on Brass. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 10	5.33	2 5.9
24	3 8	6.85	4 1.0
36	6 5	5.70	6 1.6
48	8 4	5.81	8 2.1
60	10 3	5.88	10 2.7
72	12 0	6.00	12 3.2
84	14 0	6.00	14 3.7
96	16 0	6.00	16 4.3
192	44 8	4.31	32 8.0

Weight to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
Cast Iron on Cast Iron. Area 6.75.			
lbs.	lbs. oz.		lbs. oz.
14	2 4	6.22	2 1.2
24	3 0	8.00	3 8.9
36	5 14	6.12	5 5.3
48	7 10	6.29	7 1.7
60	9 8	6.31	8 14.2
72	11 7	6.29	10 10.6
84	13 5	6.30	12 7.1
96	15 5	6.27	14 3.5
Brass sliding on Steel. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 1	6.78	2 5.9
24	3 8	6.85	4 1.0
36	5 0	7.20	6 1.6
48	7 11	6.24	8 2.1
60	9 11	6.19	10 2.7
72	11 5	6.36	12 3.2
84	13 0	6.46	14 3.7
96	15 0	6.40	16 4.3
192	28 0	6.85	32 8.0
Cast Iron on Wrought Iron. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 4	6.22	2 5.9
24	4 2	5.81	4 1.0
36	6 2	5.87	6 1.6
48	7 12	6.19	8 2.1
60	9 8	6.31	10 2.7
72	11 5	6.36	12 3.2
84	13 13	6.08	14 3.7
96	17 0	5.64	16 4.3
192	33 8	5.73	32. 8.0

Experiments on the Friction of different Metals with the weights increased from 14lbs. to 192lbs.

Weight to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
Cast Iron sliding on Soft Steel. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 2	6.59	2 5.9
24	3 10	6.62	4 1.0
36	5 7	6.62	6 1.6
48	7 2	6.73	8 2.1
60	9 8	6.31	10 2.7
72	11 9	6.22	12 3.2
84	13 9	6.19	14 3.7
96	15 5	6.26	16 4.3
192	32 0	6.00	32 8.0
Soft Steel on Soft Steel. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 0	7.00	2 5.9
24	3 7	6.98	4 1.0
36	5 4	6.85	6 1.6
48	6 13	7.04	8 2.1
60	8 11	6.90	10 2.7
72	10 5	6.98	12 3.2
84	12 2	6.92	14 3.7
96	13 12	6.98	16 4.3
192	31 8	6.09	32 8.0
Wrought Iron on Wrought Iron. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 1	6.78	2 5.9
24	3 13	6.29	4 1.0
36	5 12	6.26	6 1.6
48	7 2	6.73	8 2.1
60	9 8	6.31	10 2.7
72	11 6	6.32	12 3.2
84	12 15	6.49	14 3.7
96	14 3	6.76	16 4.3
192	27 0	7.11	32 8.0
Tin sliding on Wrought Iron. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	2 10	5.33	2 5.9
24	4 6	5.48	4 1.0
36	6 8	5.53	6 1.6
48	7 14	6.09	8 2.1
60	9 13	6.11	10 2.7
72	11 13	6.09	12 3.2
84	13 15	6.02	14 3.7
96	15 13	6.07	16 4.3
192	32 8	5.90	32 8.0

Weight to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
Tin sliding on Tin. Area 5.9.			
lbs.	lbs. oz.		lbs. oz.
14	3 10	3.86	2 5.9
24	7 8	3.20	4 1.0
36	9 8	3.78	6 1.6
48	12 13	3.74	8 2.1
60	17 7	3.44	10 2.7
72	22 2	3.25	12 3.2
84	28 8	2.94	14 3.7
96	36 0	2.66	16 4.3
192	66 8	2.88	32 8.0
Cast Iron on Hard Brass. Area 7.75.			
lbs.	lbs. oz.		lbs. oz.
14	1 11	8.29	1 12.9
24	4 0	6.00	3 1.5
36	6 0	6.00	4 10.3
48	7 13	6.14	6 3.0
60	9 0	6.66	7 11.8
72	11 0	6.54	9 4.6
84	13 2	6.40	10 13.4
96	14 8	6.62	12 6.1
Brass on Cast Iron. Area 6.75.			
lbs.	lbs. oz.		lbs. oz.
14	2 1	6.78	2 1.2
24	3 8	6.85	3 8.9
36	5 1	7.11	5 5.3
48	6 10	7.24	7 1.7
60	9 3	6.53	8 14.2
72	10 5	6.98	10 10.6
84	13 12	6.10	12 7.1
96	15 1	6.37	14 3.5
Tin sliding on Cast Iron. Area 6.75.			
lbs.	lbs. oz.		lbs. oz.
14	2 12	5.09	2 1.2
24	4 8	5.33	3 8.9
36	6 7	5.59	5 5.3
48	8 14	5.40	7 1.7
60	9 13	6.11	8 14.2
72	11 13	6.09	10 10.6
84	14 5	5.86	12 7.1
96	16 4	5.90	14 3.5

Table VIII.

A Table showing the Power required to move a Weight progressively increased until the metals abrade each other.

Weight to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
Wrought Iron on Wrought Iron. Area 6 inches.			
cwt.	cwt. qrs.		cwt.
10	2 2	4.00	1.66
12	3 1	3.69	2.00
14	4 0	3.50	2.33
16	4 3	3.36	2.66
18	5 2.5	3.20	3.00
20	7 0	2.85	3.33
22	8 1	2.66	3.66
24	9 0	2.66	4.00
26	10 1	2.53	4.33
28	11 1	2.48	4.66
30	12 1	2.44	5.00
Steel on Cast Iron. Area 6 inches.			
cwt.	cwt. qrs.		cwt.
10	3 0	3.33	1.66
12	4 0	3.00	2.00
14	4 3	2.94	2.33
16	5 2	2.90	2.66
18	6 1	2.88	3.00
20	7 0	2.85	3.33
22	7 3	2.83	3.66
24	8 2	2.82	4.00
26	9 1	2.81	4.33
28	10 0	2.80	4.66
30	10 3	2.79	5.00
32	11 2	2.78	5.33
34	12 2	2.72	5.66
36	14 2	2.48	6.00

Weight to be moved.	Weight required to move it.	Proportion.	Weight to 1 inch of area.
Wrought Iron on Cast Iron. Area 6 inches.			
cwt.	cwt. qrs.		cwt.
10	2 3	3.63	1.66
12	3 2	3.42	2.00
14	4 2	3.11	2.33
16	5 1	3.04	2.66
18	6 0	3.00	3.00
20	7 0	2.85	3.33
22	7 3	2.83	3.66
24	8 3	2.74	4.00
26	9 2	2.73	4.33
28	10 1	2.73	4.66
30	11 0	2.72	5.00
32	11 3	2.72	5.33
34	12 2	2.72	5.66
36	13 2	2.66	6.00
38	16 2	2.30	6.33
Brass on Cast Iron. Area 6 inches.			
cwt.	cwt. qrs. lbs.		cwt.
10	2 1 00	4.44	1.66
12	2 2 14	4.37	2.00
14	3 0 00	4.66	2.33
16	3 1 14	4.74	2.66
18	3 3 14	4.64	3.00
20	4 0 14	4.84	3.33
22	4 2 00	4.88	3.66
24	5 0 00	4.80	4.00
26	5 3 00	4.52	4.33
28	6 1 00	4.48	4.66
30	7 0 00	4.28	5.00
32	7 2 00	4.26	5.33
34	8 0 00	4.25	5.66
36	8 1 14	4.29	6.00
38	8 3 14	4.28	6.33
40	9 1 14	4.26	6.66
42	9 3 00	4.30	7.00
44	12 0 00	3.66	7.33

Appendix to Tables VII. and VIII.

Table showing the comparative amount of Friction of different Metals under an average pressure of from 54.25lbs. to 69.55lbs. as calculated from the foregoing experiments.

Description of Metals.	Average Weight.	Proportion.	Weight per Square Inch Area.
	lbs.		lbs. oz.
Brass on wrought iron	69.55	7.312	11 12.4
Steel upon steel	69.55	6.860	11 12.5
Brass upon cast iron	54.25	6.745	8 0.5
Brass upon steel	69.55	6.592	11 12.5
Hard brass upon cast iron	54.25	6.581	6 15.9
Wrought iron on wrought iron ..	69.55	6.561	11 12.5
Cast iron upon cast iron	54.25	6.475	8 0.5
Cast iron upon steel	69.55	6.393	11 12.5
Cast iron upon wrought iron....	69.55	6.023	11 12.5
Tin upon wrought iron	69.55	5.846	11 12.5
Brass upon brass	69.55	5.764	11 12.5
Tin upon cast iron	54.25	5.671	8 0.5
Steel upon wrought iron	69.55	5.198	11 12.4
Tin upon tin	69.55	3.305	11 12.5

Remarks on Tables VII. and VIII.

1. From the preceding experiments it appears:—that the friction of metals varies with their hardness.

2. That the hard metals have less friction than the soft ones.

3. That without unguents and within the limits of 32lbs 8oz. per square inch, the friction of hard metals against hard metals may very generally be estimated at about one-sixth of the pressure.

4. That within the limits of their abrasion the friction of metals is nearly alike.

5. That from 1.66cwt. per square inch to upwards of 6cwt. per square inch, the resistance increases in a very considerable ratio, being the greatest with steel on cast iron, and the least with brass on wrought iron, their limits being as 30, 36, 38, and 44cwt. An experiment was made with a weight of 10 tons per inch on hardened steel, which abraded.

The remarkable property of steel in hardening, and its power to resist abrasion, render it preferable to every other substance yet discovered in reducing the friction of delicate instruments, as is exemplified in the different experiments on the pendulum, and the assay and other balances recently introduced at His Majesty's Mint and the Bank of England.

The experiments of Messrs. Cavendish and Hatchett in the years 1798 and 1801 at His Majesty's Mint on the alloys, specific gravity, and comparative wear of gold coin by friction, likewise prove that friction and abrasion were less in the hard than soft metals. Philosophical Transactions for 1803, Part I.

Table IX.

Experiments on the Friction of Axles without and with Unguents.

Weight on Axle.	Weight required to move it.	Time.	Proportion.	Space passed over.
Gun Metal on Cast Iron.				
cwt.	lbs. oz.	sec.		
1	16 0	90	7.00	
2	30 0	,,	7.46	
3	44 0	,,	7.63	
4	60 12	,,	7.37	
5	112 0	80	5.00	4½ inches.
6	134 0	90	5.01	
7	After remaining 12 hrs. it took to move it 154 0	,,	5.09	
8	175 0	,,	5.12	
9	200 0	,,	5.04	
10	238 0	,,	4.70	
Yellow Brass on Cast Iron.				
cwt.	lbs. oz.	sec.		
10	272 0	90	4.11	4½ inches.
Cast Iron on Cast Iron.				
cwt.	lbs. oz.	sec.		
10	173 8	90	6.45	4½ inches.
11	228 0		5.40	
Cast Iron on Cast Iron with Black-lead.				
cwt.	lbs. oz.	sec.		
11	161 0	90	7.65	4½ inches.
Gun Metal on Cast Iron with Black-lead.				
cwt.	lbs. oz.	sec.		
11	170 0	90	7.24	4½ inches.
Yellow Brass on Cast Iron with Black-lead.				
cwt.	lbs. oz.	sec.		
1	14 12		7.59	
2	31 4		7.16	
3	47 8	90	7.07	4½ inches.
4	65 8		6.83	
5	84 0		6.66	
11	181 0		6.80	
Gun Metal on Cast Iron with Oil.				
cwt.	lbs. oz.	sec.		
11	218 8	90	5.63	4½ inches.

Experiments on the Friction of Axles without and with Unguents.

Weight on Axle.	Weight required to move it.	Time.	Proportion.	Space passed over.
Yellow Brass on Cast Iron.				
cwt.	lbs. oz.			
½	1 8		37.33	
1	3 8		32.00	
2	7 0		32.00	
3	16 8	sec. 90	20.36	4½ inches.
4	24 8		18.28	
5	29 4		19.14	
10	193 8		5.78	
11	200 12		6.13	
Cast Iron on Cast Iron.				
cwt.	lbs. oz.			
10	131 1	sec. 90	8.54	4½ inches.
11	140 0		8.80	
Cast Iron on Cast Iron with Hogslard.				
cwt.	lbs. oz.	sec.		
10	117 4	90	9.55	4½ inches.
Yellow Brass on Cast Iron.				
cwt.	lbs. oz.			
½	1 10		34.46	
1	3 1		36.57	
2	7 8	sec.	29.86	
3	23 0	90	14.60	4½ inches.
4	43 0		10.41	
5	47 8		11.78	
10	120 8		9.29	
Gun Metal on Cast Iron with Hogslard.				
cwt.	lbs. oz.	sec.		
10	130 4	90	8.59	4½ inches.
Yellow Brass on Cast Iron with Anti-Attrition Composition.				
cwt.	lbs. oz.			
1	7 8		14.93	
2	9 0		24.88	
3	10 8		32.00	
4	12 8	sec.	35.84	
5	14 8	90	38.62	4½ inches.
After remaining 41 hrs. in a state of rest with 10	it took 190 0		5.89	
Fresh composition being 10	it took only 23 8		47.65	
applied 10	again 20 0		56.00	

Experiments on the Friction of Axles without and with Unguents.

Weight on Axle.	Weight required to move it.	Time.	Proportion.	Space passed over.
Yellow Brass on Cast Iron with Tallow.				
cwt.	lbs. oz.			
1	3 1		36.57	
2	5 12	sec.	38.95	
3	8 5	90	40.42	4½ inches.
4	11 1		40.49	
5	13 12		40.72	
Yellow Brass on Cast Iron with Soft-soap.				
cwt.	lbs. oz.			
½	2 2		26.35	
1	3 8		32.00	
2	6 0		37.33	
3	9 8	90	35.36	4½ inches.
4	12 12		35.13	
5	14 12		37.96	
Yellow Brass on Cast Iron with Soft-soap and Black-lead.				
cwt.	lbs. oz.			
½	5 8		10.18	
1	9 3		12.19	
2	12 1		18.56	
3	14 4	90	23.57	4½ inches.
4	19 8		22.97	
5	23 8		23.82	

Remarks on the experiments without Unguents.

From the foregoing experiments it appears,—

That when gun metal without unguents is loaded with variable weights of from 1 to 10 cwt., friction varies nearly in the proportion of $7.\frac{1}{23}$ to $3.\frac{1}{70}$ of the pressure.

That length of time scarcely affects it.

That friction increased when yellow brass was tried.

That friction decreased when cast iron was tried.

That friction diminished still more when black-lead was used between the three different metals.

Remarks on the experiments with Unguents.

That gun metal on cast iron, with oil intervening and a weight of 10 cwt., amounted to $3.\frac{1}{83}$ of the pressure.

That when the insistent weights were diminished, the friction with oil was reduced to $27.\frac{1}{33}$, but increased with an increase of weight.

That cast iron on cast iron, under similar circumstances, showed less friction.

That the friction of cast iron on cast iron was still further diminished by hogslard.

That the friction of yellow brass on cast iron was increased by light weights and diminished by heavy weights, perhaps from being less fluid and sensible in the one case, and more capable of preventing the contact of metals in the other.

That gun metal on cast iron with hogslard gave less friction than with oil.

That yellow brass on cast iron with anti-attrition composition of black-lead and hogslard, increased friction with light weights, and greatly diminished it with heavy weights, showing extremely irregular results.

That yellow brass on cast iron with tallow gave the least friction, and may therefore be considered the best substance under the circumstances tried.

That yellow brass on cast iron with soft-soap gave the second best result, being superior to oil.

That yellow brass on cast iron with soft-soap and black-lead gave the worst result, diminishing the friction in the inverse ratio of the weight.

Conclusion.—That the diminution of friction by unguents varies as the insistent weights and natures of the unguents; the lighter the weight the finer and more fluid should be the unguents, and vice versa.

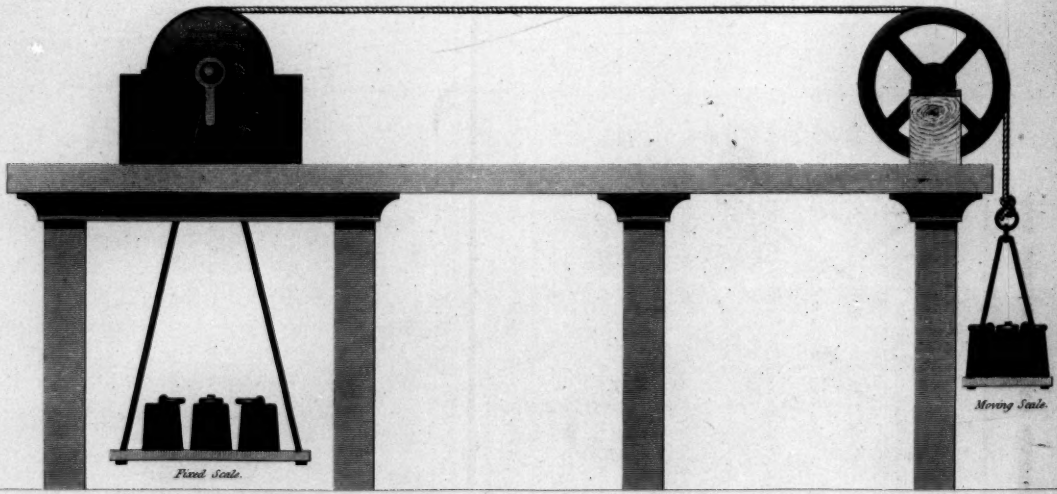
TABLE X.

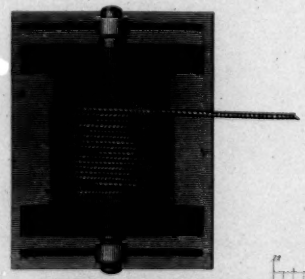
Experiments on Velocities in Friction.

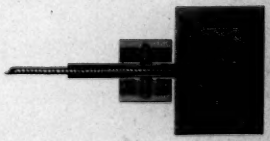
A cast iron Cylinder with two bearings of one inch wide, six inches diameter, with two side collars one-eighth of an inch deep; a rope of three-eighths of an inch diameter wound round the cylinder. Bearing surface 12½ square inches. (Plate V.)

Without Oil.

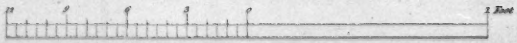
Weight in roller scale.	Weight required to move the roller.	Height fallen.	Time in falling.	Remarks.	Proportion.
lbs. oz.	lbs. oz.		Seconds.		
348 8	112 0			Four trial experiments made by decreasing weight; very unsteady motion.	3.11
300 0	112 0		5		2.67
280 0	114 0		7*		2.45
280 0	114 0		7†	* Began to grind.	2.45
280 0	228 0		4½	† Grinding increasing with stopping.	1.22
224 8	112 0		6		2.00
224 8	112 0		4½		2.00
174 8	58 0		4		3.00
174 8	58 0		4		3.00
174 8	116 0	21 feet	2		1.50
174 8	116 0		2		1.50
160 8	56 0		7		2.86
160 8	56 0		8		2.86
66 8	28 0		8		2.37
62 8	22 0		4		2.84
62 8	22 0		4		2.84
62 8	44 0		2½		1.42
62 8	44 0		2½		1.42
62 8	44 0		2½		1.42







Plan and Elevation
of the Apparatus for trying the effect of Velocities on
Friction.



Experiments with Oil.					
Weight in roller scale.	Weight required to move the roller.	Height fallen.	Time in falling.	Remarks.	Proportion.
			1st half. 2nd half. Seconds.		
lbs. oz.	lbs. oz.				
62 8	7 0		12		8.92
62 8	7 0		17½		8.92
62 8	7 0		11 22		8.92
62 8	7 0		9 18		8.92
62 8	7 0		8 16		8.92
62 8	7 0		8 16	Found the velocity too	8.92
62 8	7 0	21 feet	8½ 17	great; made an addi-	8.92
62 8	14 0		3 5	tion of 21¼lbs. making	4.46
62 8	14 0		3 5	¼cwt. in the fixed scale,	4.46
62 8	14 0		3 5	which brought it re-	4.46
84 0	14 0		3½ 7	gular. This weight produced a	6.00
84 0	14 0		3½ 7	regular velocity.	6.00
84 0	14 0		3½ 7		6.00
Experiments with Tallow.					
lbs. oz.	lbs. oz.		Seconds.		
272 8	42 0		14 28		6.48
272 8	42 0		6½ 13		6.48
272 8	42 0	21 feet	6½ 13		6.48
272 8	42 0		7 14		6.48

REMARKS.—The irregularity of the resistances observed in the first seven experiments arose from the impartial contact and consequent grinding or abrading of the surfaces. The roller having attained its bearing, and the weight in the roller-scale being diminished, the irregularity ceased, especially when oil and tallow were used.

From the result of these experiments it will be seen, that friction did not increase with an increase of velocity. The time in falling the whole height of twenty-one feet being double the time in falling half the height. These experiments were likewise illustrated (but not so satisfactorily,) by a machine somewhat similar to that of Mr. ROBERTS.—The pulley was sufficiently distant from the roller to render the angle of tension imperceptible.

Appendix to TABLE X.

Friction of the cord and weight on the axles of the iron rollers, to be deducted from the foregoing experiments.

Weight in each end.	Total of weights.	Weight required to overcome the friction of the cord.
lbs.	lbs.	lbs. oz.
56	112	4 8
112	224	7 0
168	336	11 4
224	448	14 0

Remarks on TABLE X.

The deductions to be made for the rigidity of the cord used in the foregoing experiments under variable weights, as shown in the second and third columns, are nearly as the weights simply, and are applicable to most of the cases in Table IX.

TABLE XI. Experiments on the friction of Ice.

A block of ice eighteen inches long and two inches thick, as free as possible from air-bubbles, was accurately prepared so as to present a smooth, flat surface, and was then fixed on the frame. A piece of the same block of ice, but of smaller dimensions, was accurately prepared, and made to glide with its flat surface over the bottom block, and a fine flexible silken cord attached to it as in the former experiments.

The weights in the first column indicate the insistent weights, and the weight in the second column the moveable weights. The experiments were made when the temperature of the atmosphere was about 28 degrees of FAHR.

Sixteen inches surface.				With two skates $4\frac{1}{2}$ inches long, by $\frac{1}{10}$ wide, in surface in each.		
Weight on surface.	Weight required to move it.	Proportion.	Weight on surface.	Weight required to move it.	Proportion.
lbs. oz.	lbs. oz.		lbs. oz.	lbs. oz.	
1 8	0 3	8.00	1 8	0 1	24.00
4 0	0 5	12.80	4 0	0 3	21.33
16 0	0 10	25.60	16 0	0 7	36.57
36 0	1 0	36.00	36 0	0 15	38.40
64 0	1 6	46.54	64 0	1 2	56.88
81 0	1 13	44.68	81 0	1 10	49.84
144 0	2 9	56.19	144 0	2 1	69.81
After remaining 16 hours.					
lbs. oz.	lbs. oz.				
1 8	0 3	8.00			
4 0	0 6	10.66			
16 0	0 15	17.06			
36 0	1 9	23.04			
64 0	3 2	20.48			
81 0	4 0	20.25			
144 0	6 5	22.81			

REMARKS.—From the foregoing experiments it appears, that with ice on ice, friction diminishes with an increase of weight, but does not seem to observe any regular law with regard to that increase.

Table XII. Experiments on the Friction of Hide Leather.

Twelve pieces of hide leather were placed parallel to each other in a wooden box, with one side loose so as to admit of being adjusted according to the number of pieces of leather; a bolt was then passed through the whole, and a nut screwed on the end of the bolt so as to compress the pieces of leather together and permit them to act on edge as one uniform surface; which surface was increased or diminished by putting in or taking out some of the pieces of leather and screwing up the nut as before.

Friction of 9 square inches of leather soaked in water, moving over a plate of iron.

7 lbs. barely kept in motion 30 lbs. after starting with the hand. After remaining 5 minutes it took to start it 29 lbs. 28 lbs. barely kept in motion 64 lbs. after starting it, and after remaining one minute it took 42 lbs. to start it.

Surface $1\frac{1}{2}$ by 3 inches, equal to $4\frac{1}{2}$ inches area.

$8\frac{1}{2}$ lbs. barely kept in motion 36 lbs. after starting it. After remaining 5 minutes it took 21 lbs. to start it. 21 lbs. barely kept in motion 64 lbs. after starting it. After remaining 5 minutes it took 38 lbs. to start it.

Friction of Hide Leather moving dry over a surface of Cast Iron.

Weight on Surface.	Weight required to move it.	Proportion.	Space passed over.	Time.	Weight per inch Area.	Weight on Surface.	Weight required to move it.	Proportion.	Space passed over.	Time.	Weight per inch Area.
Area of surface 9 square inches.						Area of surface $4\frac{1}{2}$ square inches.					
lbs.	lbs. oz.		inches.	sec.	lbs.	lbs.	lbs. oz.		inches.	sec.	lbs.
6	1 8	4.0			.66	6	1 2	5.33			1.33
7	1 12	4.0			.77	7	1 5	5.33			1.55
8	2 0	4.0	18	18	.88	8	1 9	5.12	18	18	1.77
36	8 12	4.1			4.00	36	7 3	5.00			8.00
49	12 0	4.0			5.44	49	9 5	5.26			10.88
64	16 0	4.0			7.11	64	13 10	4.69			14.22
Area of surface $6\frac{3}{4}$ square inches.						Area of surface $2\frac{1}{4}$ square inches.					
lbs.	lbs. oz.		inches.	sec.	lbs.	lbs.	lbs. oz.		inches.	sec.	lbs.
6	1 4	4.80			.88	6	1 1	5.64			2.66
7	1 8	4.66			1.03	7	1 3	5.89			3.11
8	1 12	4.57	18	18	1.18	8	1 8	5.33	18	18	3.55
36	7 4	4.96			5.33	36	7 1	5.09			16.00
49	11 0	4.45			7.25	49	9 1	5.40			21.77
64	14 0	4.57			9.48	64	13 2	4.87			28.44

REMARKS.—The friction of hide leather soaked in water appears to be greatly increased by time and weight. This circumstance explains the enormous friction evinced in the pistons of pumps when first put in motion. When the leather is not soaked, the resistance varies from one $\frac{1}{4}$th to nearly one $\frac{1}{3}$th of the pressure, and is diminished (cæteris paribus) by a diminution of surface.

XIII. On the Friction of Stones.

Rondelet found that stones well dressed required angles of from 28° to 36° before they commenced gliding*. Perronet observed them to vary from 39° to 40°†. The granite voussoirs of the arches of the New London Bridge having their beds well faced and dressed without mortar, generally commence gliding at angles of from 33° to 34°. But with a bed of fresh and finely ground mortar interposed, the pressure on the centring commences at angles of from 25° to 26°. In other cases of arches where sandstones, such as Bramley Fall and Whitby were employed, and their beds faced and dressed as usual, the angle of gliding was found to vary from 35° to 36°. But with mortar interposed the angle generally varied from 33° to 34°.

It results from these and other experiments, that friction, by absorbing part of the horizontal thrust, is a most powerful assistant in maintaining the equilibrium of arches, and enables us to determine with something like precision the allowances due to theory.

In general, stones which have a fine grain and uniform texture, and are sonorous and heavy, resist abrasion in proportion to their hardness; and in some experiments of Morisot‡, granite resists abrasion twelve times more than lias, whilst the former only possesses a repulsive power three times greater than the latter.

The experiments of Boistard give 0.78 for the friction of hard calcareous stones§.

XIV. On the Friction of Machines.

1. 21 cwt. (suspended at each extremity of a chain passing over two cast iron sheaves of 2 feet diameter with wrought iron axles, working in brass bearings oiled, and 12 feet 10 inches apart) was disturbed by 3 cwt. or $\frac{1}{7}$th of the total weight. Another double purchased crane indicated $\frac{1}{8}$th.

2. A double purchased crane having a weight of 7057lbs. suspended to it indicated 7.62 for the friction. Another double purchased crane indicated $\frac{1}{8}$th.

* L'Art de Batir. Tome iii.
† Mémoire sur le Cintrement et Décintrement des Ponts.
‡ Morisot, Tome iv.
§ Recueil d'Experiences et d'Observations &c.

In an experiment made on one of the corn mills recently erected for His Majesty's victualling department at Deptford, it required $\frac{1}{10}$ of the weight of the mass to overcome the inertia and friction of the bearings and tangential surfaces. In this instance the pressures of the different parts of the machine varied from 28lbs. to 8 cwt. per inch area, and the velocities of the surfaces from 50 feet to 120 feet per minute.

REMARKS.—It has been customary to deduct one fourth of the power expended for friction. This allowance may maintain in machines newly set in motion. When the bearings have been equalized and the rubbing surfaces extended by the abrasion of the irregularities, the friction will be diminished and the movements of the machine be more steady. But when the bearings are properly proportioned to the weight of the parts of a machine, and their surfaces kept from contact by unguents, a much less allowance may be made.

Several experiments were made by giving motion to a fly wheel and a grindstone of known weights and revolutions in a given time, and then counting the revolutions after being detached from the power; but owing to the resistance of the air, and the bearings being too small, the results were unsatisfactory.

TABLE XV. Showing the amount of friction (without unguents) of different substances, the insistent weight being 36lbs. and within the limits of abrasion of the softest substance.

	Parts of the whole weight.		Parts of the whole weight.
Steel on ice	69.81	Cast iron on wrought iron	5.87
Ice on ice	36.00	Brass on brass	5.70
Hard wood on hard wood	7.73	Tin on cast iron	5.59
Brass on wrought iron	7.38	Tin on wrought iron	5.53
Brass on cast iron	7.11	Soft steel on wrought iron	5.28
Brass on steel	7.20	Leather on iron	4.00
Soft steel on soft steel	6.85	Tin on tin	3.78
Cast iron on steel	6.62	Granite on granite	3.30
Wrought iron on wrought iron	6.26	Yellow deal on yellow deal	2.88
Cast iron on cast iron	6.12	Sand-stone on sand-stone	2.75
Hard brass on cast iron	6.00	Woollen cloth on woollen cloth	2.30

These results are collected from the different Tables, but the comparison may be made by selecting other values within the limits of abrasion for a minimum.

General Conclusions.

From what has been stated hitherto it is obvious,—

1st. That the laws which govern the retardation of bodies gliding over each other are as the nature of those bodies.

2nd. That with fibrous substances, such as cloth, &c. friction is increased by surface and time, and diminished by pressure and velocity.

3rd. That with harder substances, such as woods, metals, and stones, and within the limits of abrasion, the amount of friction is as the pressure directly, without regard to surface, time, or velocity.

4th. That with dissimilar substances gliding against each other, the measure of friction will be determined by the limit of abrasion of the softer substance.

5th. That friction is greatest with soft, and least with hard substances.

6th. That the diminution of friction by unguents is as the nature of the unguents, without reference to the substances moving over them.

The very soft woods, stones, and metals, approximate to the laws which govern the fibrous substances.

In comparing the present experiments with those of COULOMB, the discordances found to exist relate principally to time. The limited pressures (varying from 1 to 45lbs. per square inch) under which his experiments were made, account in some degree for the anomaly. But in many of the minor, and in the general results, they will be found to coincide.

The subject might be illustrated still further by detailing the results of other experiments on the motions of machines, on the friction of solids revolving in fluids, and the descent of carriages down inclined planes. But as the present inquiry principally relates to the friction of attrition of solids, and as the experiments last mentioned have not been sufficiently matured to arrive at the necessary deductions, it only remains to conclude by expressing a hope, that the data now furnished will in some degree enlarge the bounds of our knowledge on this subject, interesting as one of philosophical inquiry, and intimately connected with every branch of the mechanical arts.

XVI.—*An attempt to rectify the inaccuracy of some logarithmic formulæ.* By JOHN THOMAS GRAVES, *of the Inner Temple, Esq.* *Communicated by* JOHN FREDERICK WILLIAM HERSCHEL, *Esq. V.P.*

Read December 18, 1828.

FROM the recent researches [Note A.] of MM. POISSON and POINSOT on angular section, and their discovery of error in trigonometrical formulæ usually considered complete, my attention has been drawn to analogous incorrectness in logarithmic series. Accordingly, the end proposed in the present investigation is the exhibition in an amended form of two fundamental developments, as the principles employed in their establishment admit of application in expanding by different methods various similar functions, and tend to elucidate other parts of the exponential theory.

Let $a^x = y$. [1]

It is proposed to exhibit correct developments;

I. Of y in terms of a and x;

II. Of x in terms of a and y;

the corresponding formulæ hitherto given being incomplete; viz.*

I. $$y = 1 + x\,la \ldots + \frac{(x\,la)^n}{1.2 \ldots n} \ldots \quad [2]$$

II. $$x, \text{ when } y \text{ is positive, } = \frac{\sqrt{-1}\,2i\pi + ly}{la} \quad [3]$$

Some authors, for the case when y is negative, have provided for x the formula

$$\frac{\sqrt{-1}(2i+1)\pi + l - y}{la} \quad [4]$$

The notation above used will be adhered to, and requires to be explained. i denotes 0, or any integer positive or negative, and π the ratio of the circumference of a circle to its diameter. la is intended to designate the tabular

* Lacroix, "Traité du Calcul différentiel et intégral:" Introduction, Art. 25, 27, 28, 31.

Neperian logarithm of a, which logarithm is a quantity assignable only in the case when a is positive, and may then be found from the development

$$-2\left\{\frac{1-a}{1+a}\cdots+\frac{1}{2n+1}\left(\frac{1-a}{1+a}\right)^{2n+1}\cdots\right\}. \qquad [5]$$

Independently of the circumstance that neither of these formulæ for y and x provides for the case when a is negative or impossible, and that neither [3] nor [4] provides for the case when y is impossible, their incompleteness will appear from what follows.

That [2] is incomplete is primâ facie obvious, from the known fact that when x is a rational fraction, a^x has as many values as there are units in the denominator of x reduced to its lowest terms, whereas [2] never exhibits more than one value.

Thus, $e^{\frac{1}{2}}$ (e being the Neperian base and $1e = 1$) has two values, viz: $+\sqrt{e}$ and $-\sqrt{e}$, whereas

$$1+\tfrac{1}{1}\cdots+\frac{1}{1.2\ldots n}\left(\tfrac{1}{1}\right)^n\cdots$$

represents the value $+\sqrt{e}$ only.

The imperfection of [3] and [4] arises from the imperfection of [2], of which [3] and [4] are reverted solutions.

Thus, as one of the values of $e^{\frac{1}{2}} = -\sqrt{e}$, $\frac{1}{2}$ is a Neperian logarithm of $-\sqrt{e}$, but yet, if in [4] $-\sqrt{e}$ be substituted for y, and e for a, the resulting formula, viz.

$$\frac{\sqrt{-1}(2i+1)\pi+1\sqrt{e}}{1e} \quad \text{or} \quad \sqrt{-1}(2i+1)\pi+\tfrac{1}{2}$$

comprises, whatever value be given to i, only imaginary quantities, among which, of course, $\frac{1}{2}$ cannot be found.

For the purpose of developing y and x correctly, adopting the equation

$$\mathrm{f}\theta = \cos\theta + \sqrt{-1}\sin\theta \qquad [6]$$

it will be useful to possess two preliminaries;

1st, a development of $\mathrm{f}\theta$;

2nd, a development of $\mathrm{f}^{-1}\theta$;

as it will appear that upon the form of these developments depend the desired ones of y and x.

(By $\mathrm{f}^{-1}\theta$ is to be understood, according to the notation of Mr. Herschel, every such quantity q, that $\mathrm{f}q = \theta$).

Postulates.

To obviate the necessity of interrupting the course of the argument hereafter, it may be satisfactory to enumerate the principal truths immediately connected with our subject and not immediately evident, which will be taken for granted in this paper.

For their support, the authority of Dr. LARDNER's Trigonometry, Part III. Sections 1 and 2, may be referred to.

EULER's development of $f\theta$, or

$$f\theta = 1 + \sqrt{-1}\,\theta \ldots + \frac{(\sqrt{-1}\,\theta)^n}{1.2 \ldots n} \ldots \quad [7]$$

DE MOIVRE's theorem, or

$$f(x\theta) = \text{a value of } (f\theta)^x \quad [8]$$

$$f^{-1}f\theta = 2i\pi + \theta \quad [9]$$

DE MOIVRE's theorem as extended by M. POINSOT, or

$$f\{x(2i\pi + \theta)\} = (f\theta)^x \quad [10]$$

$$f(\theta + h) = f\theta \,.\, fh \quad [11]$$

Subsidiary division.

1st, To possess a development of $f\theta$.

The development of EULER [7] is accurate and sufficient.

2nd, It remains to obtain a development of $f^{-1}\theta$.

Differentiating [6] we obtain

$$\frac{df\theta}{d\theta} = \sqrt{-1}\,(\cos\theta + \sqrt{-1}\sin\theta) \text{ or } \sqrt{-1}\,f\theta \quad [12]$$

Substituting in [12] $f^{-1}\theta$ for θ, we obtain

$$\frac{df f^{-1}\theta}{df^{-1}\theta} = \sqrt{-1}\,f f^{-1}\theta\,; \text{ or since } f f^{-1}\theta = \theta\,; \quad \frac{d\theta}{df^{-1}\theta} = \sqrt{-1}\,\theta.$$

Hence we find

$$\frac{df^{-1}\theta}{d\theta} = (\sqrt{-1}\,\theta)^{-1}. \quad [13]$$

It is evident by [13], that when θ becomes $= 0$, $\frac{df^{-1}\theta}{d\theta}$ becomes infinite, and

consequently it is impossible to develop $f^{-1}\theta$ according to the ascending integral powers of θ. Let us then proceed to develop according to the ascending powers of $1-\theta fc$; (c being a constant, and introduced—be it remarked in advance—on account of the power it possesses, if properly chosen, of rendering the intended development of $f^{-1}\theta$ convergent.)

To effect this purpose, let

$$1-\theta fc=\omega \qquad [14]$$

Hence

$$\theta=(1-\omega)(fc)^{-1}\text{; or since, by [8], } (fc)^{-1}=f-c\text{; } \theta=(1-\omega)f-c:$$

Accordingly, after substituting in [13] $(1-\omega)f-c$ for θ, and therefore $-\omega f-c\,d\omega$ for $d\theta$, we find

$$\frac{d f^{-1}\{(1-\omega)f-c\}}{d\omega}=\sqrt{-1}\,(1-\omega)^{-1}$$

Hence, continuing to derive the successive differential coefficients, we obtain

$$\frac{d^n f^{-1}\{(1-\omega)f-c\}}{d\omega^n}=\sqrt{-1}.1.2\ldots n-1.(1-\omega)^{-n}$$

Hence, evidently,

$$\left(\frac{d^n f^{-1}\{(1-\omega)f-c\}}{d\omega^n}\right)=\sqrt{-1}.1.2\ldots n-1 \qquad [15]$$

(by the notation $\left(\frac{d^n f^{-1}\{(1-\omega)f-c\}}{d\omega^n}\right)$ being designated the value which $\frac{d^n f^{-1}\{(1-\omega)f-c\}}{d\omega^n}$ acquires, when $\omega=0$.)

Also, by [9], $\left(f^{-1}\{(1-\omega)f-c\}\right)$ or $f^{-1}f-c=2i\pi-c$ [16]

But, by MACLAURIN'S theorem,

$$f^{-1}\{(1-\omega)f-c\}=\left(f^{-1}\{(1-\omega)f-c\}\right)\ldots+\frac{\left(\frac{d^n f^{-1}\{(1-\omega)f-c\}}{d\omega^n}\right)}{1.2\ldots n}\omega^n\ldots$$

Substituting for the successive terms of this equation their values derived from [16] and [15], we obtain

$$f^{-1}\{(1-\omega)f-c\}=2i\pi-c+\sqrt{-1}\left(\omega\ldots+\frac{\omega^n}{n}\ldots\right) \qquad [17]$$

Replacing, in [17], ω by $1 - \theta f c$ (see [14]), and therefore $(1 - \omega)$ f $- c$ by θ, we obtain finally the required development; viz.

$$f^{-1}\theta = 2i\pi - c + \sqrt{-1}\left\{(1 - \theta f c)\ldots + \frac{(1 - \theta f c)^n}{n}\ldots\right\} \text{ [Note B.]} \quad [18]$$

Having advanced thus far, it will now be easy to fulfil our original intention.

General division.

I. To develop y in terms of a and x.

Let $$a = f\theta \quad [19]$$

Then by [10], a^x or $(f\theta)^x = f\{x(2i\pi + \theta)\}$

But by [9], $2i\pi + \theta = f^{-1}f\theta$ or (see [19]) $f^{-1}a$

Hence a^x, or (see [1]) $y = f(x f^{-1} a)$ [Note C.] [20]

Hence, expanding $f(x f^{-1} a)$ by formula [7], we obtain,

$$y = 1 + \sqrt{-1}\,x f^{-1} a\ldots + \frac{(\sqrt{-1}\,x f^{-1} a)^n}{1.2\ldots n}\ldots \quad [21]$$

II. To develop x in terms of a and y.

Solving [20], we obtain, $$x = \frac{f^{-1} y}{f^{-1} a} \text{ [Note D.]} \quad [22]$$

Hence, developing by formula [18],

$$x = \frac{2i\pi - \dot{c} + \sqrt{-1}\left\{(1 - y f \dot{c})\ldots + \frac{1}{n}(1 - y f \dot{c})^n\ldots\right\}}{2i\pi - c + \sqrt{-1}\left\{(1 - a f c)\ldots + \frac{1}{n}(1 - a f c)^n\ldots\right\}} \quad [23]$$

($\dot{i}$ and $\dot{c}$ are dotted underneath, to show that when rendered determinate, their individual values may differ from those of i and c.)

[21] and [23] may now be compared with [2], [3], and [4].

Remarks on the application of the preceding theory.

From the foregoing principles many collateral deductions may be inferred. For instance, they present a solution of difficulties and illustrate peculiarities appertaining to the theory of the logarithms of negative quantities. Directed to geometry, they advance into an almost uninvestigated part of analysis, by conducing to trace the form and evolve the properties of curves (if figures,

consisting generally of discontinuous points, can accurately be called curves), whose equations involve exponential functions. By their means also, various differential and other formulæ usually exhibited in logarithmic treatises may be rendered complete. An extended pursuit of these objects would exceed the limits of the present design; but to explain briefly the mode of procedure employed in application of the preceding general results, an Appendix is subjoined, containing a few examples.

APPENDIX.

§ 1. The constant c might appear to be needlessly introduced, if its necessity to insure the convergence (and universal accuracy [Note E.]) of the series [18] were not plain from what follows.

Differentiating n terms of the series [18] there results,

$$-\sqrt{-1}\,\mathrm{f}c\left\{1+(1-\theta\,\mathrm{f}c)\ldots+(1-\theta\,\mathrm{f}c)^{n-1}\right\}\mathrm{d}\theta$$

which, as is evident on multiplying by $1-(1-\theta\,\mathrm{f}c)$,

$$=-\sqrt{-1}\,\mathrm{f}c\left\{\frac{1-(1-\theta\,\mathrm{f}c)^{n}}{1-(1-\theta\,\mathrm{f}c)}\right\}\mathrm{d}\theta \text{ or } (\sqrt{-1}\,\theta)^{-1}\left\{1-(1-\theta\,\mathrm{f}c)^{n}\right\}\mathrm{d}\theta \quad [24]$$

This expression, if the series [18] be convergent, or, carried to infinity, be numerically equivalent to $\mathrm{f}^{-1}\theta$, ought, as n is increased without limit, to approach indefinitely to $\mathrm{d}\,\mathrm{f}^{-1}\theta$, or (see [13]) $(\sqrt{-1}\,\theta)^{-1}\,\mathrm{d}\,\theta$; but, on referring to [24], it is obvious that such can be the case only where c is so assumed, that, n being supposed to increase without limit, $(1-\theta\,\mathrm{f}c)^{n}$ shall approach indefinitely to 0.

Were c neglected, or, in other words, taken $=0$, and therefore (see [6]) $\mathrm{f}c=1$, θ would not always necessarily lie between such limits that $(1-\theta)^{n}$ should possess this property; but a quantity $\mathrm{f}c$ is, in any case, supposable, which will insure for $(1-\theta\,\mathrm{f}c)^{n}$ the required essential, whatever, at the time, be the value of θ.

§ 2. If a^{x} have among its values two quantities differing only in sign, x must be a rational fraction with, in its lowest terms, an even denominator. [Note F.]

By [20] all the values of a^{x} are expressed by $\mathrm{f}(x\,\mathrm{f}^{-1}a)$. Any determined

value must, therefore, be expressible by $f(x\grave{f}^{-1}a)$, where $\grave{f}^{-1}a$ is a determined value of $f^{-1}a$. Moreover, by [9], the expressions $f^{-1}a$, and $2i\pi+\grave{f}^{-1}a$, are co-extensive. Now a^x having two values which differ only in sign, let one of them $= f(x\grave{f}^{-1}a)$; then (since $f\pi=-1$) the other will $= f\pi.f(x\grave{f}^{-1}a)$ or (see [11]) $f(\pi+x\grave{f}^{-1}a)$. The supposition is that $f(\pi+x\grave{f}^{-1}a) =$ one of the values of a^x or $f\{x(2i\pi+\grave{f}^{-1}a)\}$. Hence, by [9], one of the quantities $2i\pi+\pi+x\grave{f}^{-1}a$ must $=$ one of the quantities $x(2i\pi+\grave{f}^{-1}a)$.

Hence x must $=$ one of the quantities $\frac{2i+1}{2i}$, a formula comprising all rational fractions, which, in their lowest terms, have even denominators.

§ 3. $f^{-1}(\theta h)=f^{-1}\theta+f^{-1}h$ [Note G.].

By [11], $f(f^{-1}\theta+f^{-1}h)=ff^{-1}\theta.ff^{-1}h$ or θh.

Hence, $f^{-1}\theta h=f^{-1}\theta+f^{-1}h$. Q. E. D.

§ 4. On the Neperian logarithms of positive numbers.

Developing by [7], it appears that $f-\sqrt{-1}=1+1\ldots+\frac{1}{1.2\ldots n}\ldots=e$ the Neperian base.

Hence, by [9], $\quad f^{-1}e=2i\pi-\sqrt{-1}$.

Hence, by [22], the Neperian logarithms of k^2 are expressed by

$$\frac{f^{-1}k^2}{2i\pi-\sqrt{-1}} \qquad [25]$$

Now, by § 3, $\quad f^{-1}\frac{2K^2}{1+K^2}=f^{-1}\frac{2}{1+K^2}+f^{-1}K^2$.

Hence $\quad f^{-1}K^2=f^{-1}\frac{2K^2}{1+K^2}-f^{-1}\frac{2}{1+K^2}$

And, K^2 being positive, (in the formulæ of this paper capital letters will be used to denote real quantities) $1-\frac{2K^2}{1+K^2}$ or $\frac{1-K^2}{1+K^2}$ and $1-\frac{2}{1+K^2}$ or $-\frac{1-K^2}{1+K^2}$ must evidently both lie between 1 and -1.

Hence it is plain that $\left(1-\frac{2K^2}{1+K^2}\right)^n$ and $\left(1-\frac{2}{1+K^2}\right)^n$ will both approach indefinitely to 0, as n increases without limit.

Hence, by § 1, constants may be dispensed with in the developments according to formula [18] of $f^{-1}\frac{2K^2}{1+K^2}$ and $f^{-1}\frac{2}{1+K^2}$.

We have, therefore,

$$f^{-1}\frac{2K^2}{1+K^2} = 2i\pi + \sqrt{-1}\left\{\frac{1-K^2}{1+K^2}\cdots + \frac{1}{2n}\left(\frac{1-K^2}{1+K^2}\right)^{2n} + \frac{1}{2n+1}\left(\frac{1-K^2}{1+K^2}\right)^{2n+1}\cdots\right\}$$

and

$$f^{-1}\frac{2}{1+K^2} = 2i\pi - \sqrt{-1}\left\{\frac{1-K^2}{1+K^2}\cdots - \frac{1}{2n}\left(\frac{1-K^2}{1+K^2}\right)^{2n} + \frac{1}{2n+1}\left(\frac{1-K^2}{1+K^2}\right)^{2n+1}\cdots\right\}$$

Hence $f^{-1}K^2$ or $f^{-1}\frac{2K^2}{1+K^2} - f^{-1}\frac{2}{1+K^2}$

$$= 2(i-i)\pi + 2\sqrt{-1}\left\{\frac{1-K^2}{1+K^2}\cdots + \frac{1}{2n+1}\left(\frac{1-K^2}{1+K^2}\right)^{2n+1}\cdots\right\}$$

Hence the Neperian logarithms of K^2 are

$$\frac{2i\pi + 2\sqrt{-1}\left\{\frac{1-K^2}{1+K^2}\cdots + \frac{1}{2n+1}\left(\frac{1-K^2}{1+K^2}\right)^{2n+1}\cdots\right\}}{2i\pi - \sqrt{-1}} \quad \text{[Note H.]} \qquad [26]$$

Corollary. When i and i are both $= 0$, this expression reduces itself to

$$-2\left\{\frac{1-K^2}{1+K^2}\cdots + \frac{1}{2n+1}\left(\frac{1-K^2}{1+K^2}\right)^{2n+1}\cdots\right\}$$

which is the tabular Neperian logarithm (see [5]) of K^2. Let it be designated by $1K^2$. On comparing [25] and [26], it appears, by [9], that $-\sqrt{-1}\,1K^2$ is one of the values of $f^{-1}K^2$.

Hence we have the equation

$$f(-\sqrt{-1}\,1k^2) = k^2 \qquad [27]$$

§ 5. To separate the real and imaginary parts of $f^{-1}\theta$.

θ in its most general form $= R + \sqrt{-1}\,S$.—(*See* LACROIX "*Traité*," *&c. Introd.* 87.)

On inspecting a circle whose radius is supposed to be $= 1$, it will be obvious that for all arcs whose magnitude lies between π and $-\pi$, the arc and sine at any time are either both positive or both negative. Suppose therefore such an arc to have for cosine the quantity $\frac{R}{\sqrt{R^2+S^2}}$, then will its sine or $\pm\sqrt{1-\frac{R^2}{R^2+S^2}} = \frac{S}{\sqrt{R^2+S^2}}$, as long as the arc and S have the same sign.

Now let $\grave{c}os^{-1}\frac{R}{\sqrt{R^2+S^2}}$ (characterized, to distinguish it from any of the other values of $\cos^{-1}\frac{R}{\sqrt{R^2+S^2}}$) be the arc, when radius = 1, in the first positive or negative semicircle, according as S is positive or negative, whose cosine $=\frac{R}{\sqrt{R^2+S^2}}$; (as $\frac{R}{\sqrt{R^2+S^2}}$ always lies between 1 and − 1, it is evident that such an arc $\grave{c}os^{-1}\frac{R}{\sqrt{R^2+S^2}}$ is always assignable) then, by what has been premised, will its sine $=\frac{S}{\sqrt{R^2+S^2}}$.

Hence

$$f\grave{c}os^{-1}\frac{R}{\sqrt{R^2+S^2}}=\frac{R+\sqrt{-1}\,S}{\sqrt{R^2+S^2}}$$

Again, let $l\sqrt{R^2+S^2}$ designate the tabular Neperian logarithm of $\sqrt{R^2+S^2}$; then, by [27], will

$$f(-\sqrt{-1}\,l\sqrt{R^2+S^2})=\sqrt{R^2+S^2}$$

Hence

$$f\grave{c}os^{-1}\frac{R}{\sqrt{R^2+S^2}}.f(-\sqrt{-1}\,l\sqrt{R^2+S^2}) \text{ or (see [11])}$$

$$f(\grave{c}os^{-1}\frac{R}{\sqrt{R^2+S^2}}-\sqrt{-1}\,l\sqrt{R^2+S^2})=R+\sqrt{-1}\,S$$

Hence, by [9],

$$f^{-1}(R+\sqrt{-1}\,S) \text{ or } f^{-1}\theta=2i\pi+\grave{c}os^{-1}\frac{R}{\sqrt{R^2+S^2}}-\sqrt{-1}\,l\sqrt{R^2+S^2}\ [28]$$

in which expression the real and imaginary parts of $f^{-1}\theta$ are separated.

Corollary. I may remark that from the ambiguity of $d\cos^{-1}\theta$, which $=\mp\sqrt{1-\theta^2}\,d\theta$, the arcs in odd positive and even negative semicircles whose cosines $=\theta$, a quantity between 1 and − 1, will be found on development to be represented by

$$2i\pi+\frac{\pi}{2}-\theta\ldots-\frac{1^2.3^2\ldots(2n-1)^2}{1.2\ldots2n.2n+1.}\theta^{2n+1}\ldots$$

Similarly, the arcs in odd negative and even positive semicircles whose cosines $=\theta$, are represented by

$$2i\pi-\frac{\pi}{2}+\theta\ldots+\frac{1^2.3^2\ldots(2n-1)^2}{1.2\ldots2n.2n+1.}\theta^{2n+1}\ldots$$

As

$$\frac{\pi}{2}-\theta\ldots-\frac{1^2.3^2\ldots(2n-1)^2}{1.2\ldots2n.2n+1.}\theta^{2n+1}\ldots$$

is a value of $\cos^{-1}\theta$, which is always less than π (it being recollected that θ is a quantity between 1 and -1) it follows that the particular value of $\cos^{-1}\frac{R}{\sqrt{R^2+S^2}}$, which I denote by $\grave{\cos}^{-1}\frac{R}{\sqrt{R^2+S^2}}$

$$= \frac{S}{\sqrt{S^2}}\left\{\frac{\pi}{2} - \frac{R}{\sqrt{R^2+S^2}} \cdots - \frac{1^2.3^2\ldots(2n-1)^2}{1.2\ldots 2n.2n+1.}\left(\frac{R}{\sqrt{R^2+S^2}}\right)^{2n+1}\cdots\right\} \qquad [29]$$

§ 6. In the equation $x^{A+\sqrt{-1}B} = y$, to determine what real values x may possess, so that in each case a corresponding value of y may likewise be real.—[Note I.]

By [20],

$$y = f\{(A+\sqrt{-1}B)f^{-1}x\}$$

By [28],

$$f^{-1}x = 2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}} - \sqrt{-1}\,l\sqrt{x^2}$$

Hence

$$y = f\left\{(A+\sqrt{-1}B)(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}} - \sqrt{-1}\,l\sqrt{x^2})\right\}$$

or

$$f\left\{A\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) + Bl\sqrt{x^2} + \sqrt{-1}\left[B\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) - Al\sqrt{x^2}\right]\right\}$$

or (see [11]).

$$f\left\{A\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) + Bl\sqrt{x^2}\right\}.f\left\{\sqrt{-1}\left[B\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) - Al\sqrt{x^2}\right]\right\}$$

In this expression the factor

$$f\left\{\sqrt{-1}\left[B\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) - Al\sqrt{x^2}\right]\right\}$$

is always real, as is evident on developing by [7].

Hence, that some y may be real, the other factor, viz.

$$f\left\{A\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) + Bl\sqrt{x^2}\right\}$$

must also have some real value.

Hence (see [6]) some one, at least, of the quantities

$$\sin\left\{A\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) + Bl\sqrt{x^2}\right\}$$

must $= 0$.

Hence some value of $\sin^{-1} 0$ or $i\pi$ must be to be found among

$$A\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right) + B1\sqrt{x^2}$$

Hence

$$1\sqrt{x^2} \text{ must } = \text{ some quantity } \frac{i\pi - A\left(2i\pi + \cos^{-1}\frac{x}{\sqrt{x^2}}\right)}{B} \qquad [30]$$

x is either positive or negative.

When x is positive, $\cos^{-1}\frac{x}{\sqrt{x^2}} = \cos^{-1} 1$ or 0.

When x is negative, $\cos^{-1}\frac{x}{\sqrt{x^2}} = \cos^{-1} -1$ or $\pm\pi$.

Hence and from [30] it follows, that, for y as well as x to be real, x must = one of those quantities whose tabular Neperian logarithms are

$$\frac{i - 2iA}{B}\pi$$

or one of the negatives of those quantities whose tabular Neperian logarithms are

$$\frac{i - (2i \pm 1)A}{B}\pi$$

Hence x must = one of the quantities

$$f\left\{\left(i - \sqrt{-1}\,\frac{i - iA}{B}\right)\pi\right\} \qquad [31]$$

a formula, which, as appears by [11] and [27], comprises all the quantities that respectively fulfil the conditions above stated.

Corollary. On retracing our steps under the guidance of formula [31], it would not be difficult to prove, among others, the following theorems, viz.

1st. When $B = 0$, for x to be negative and y real, A must be a rational fraction with, in its lowest terms, an odd denominator.

2nd. When $B = 0$, and A is a rational fraction, which, in its lowest terms, $= \frac{m}{n}$, the number of real values of y that can correspond with a real x will be one or two, according as n is odd or even.

3rd. In general, when A is irrational, y can have only one real value consistently with the simultaneous reality of an x.

4th. When B is not $= 0$ and A is rational, y, in every case when it has one real value corresponding to a real x, has an infinite number.

§ 7. On the orders and ranks of logarithms.

In [22] let $y = \mathrm{R} + \sqrt{-1}\,\mathrm{S}$ and $a = \mathrm{A} + \sqrt{-1}\,\mathrm{B}$; then, by [28], will

$$\frac{\mathrm{f}^{-1}y}{\mathrm{f}^{-1}a} \text{ or } x = \frac{2i\pi + \cos^{-1}\frac{\mathrm{R}}{\sqrt{\mathrm{R}^2+\mathrm{S}^2}} - \sqrt{-1}\,l\sqrt{\mathrm{R}^2+\mathrm{S}^2}}{2i\pi + \cos^{-1}\frac{\mathrm{A}}{\sqrt{\mathrm{A}^2+\mathrm{B}^2}} - \sqrt{-1}\,l\sqrt{\mathrm{A}^2+\mathrm{B}^2}} \qquad [32]$$

When I have thus separated respectively the real and imaginary parts of the numerator and denominator of [22], upon assigning particular values, $\grave{\imath}$ and $\grave{\imath}$, to i and i in [32], I would indicate the order of a logarithm by the $\grave{\imath}$ in the denominator, and the rank it bears in that order by the $\grave{\imath}$ in the numerator; e. g. I would say of the resulting x that, in the base a, it was the $\grave{\imath}$th logarithm of y of the $\grave{\imath}$th order.

By [20], all the values of $(\mathrm{A} + \sqrt{-1}\,\mathrm{B})^x$ are comprised in the formula

$$\mathrm{f}\left\{x\,\mathrm{f}^{-1}(\mathrm{A} + \sqrt{-1}\,\mathrm{B})\right\}$$

or, (see [28])

$$\mathrm{f}\left\{x\left(2i\pi + \cos^{-1}\frac{\mathrm{A}}{\sqrt{\mathrm{A}^2+\mathrm{B}^2}} - \sqrt{-1}\,l\sqrt{\mathrm{A}^2+\mathrm{B}^2}\right)\right\}$$

When, in this formula, i assumes the particular value $\grave{\imath}$, I would denominate

$$\mathrm{f}\left\{x\left(2\grave{\imath}\pi + \cos^{-1}\frac{\mathrm{A}}{\sqrt{\mathrm{A}^2+\mathrm{B}^2}} - \sqrt{-1}\,l\sqrt{\mathrm{A}^2+\mathrm{B}^2}\right)\right\}$$

the $\grave{\imath}$th value of $(\mathrm{A} + \sqrt{-1}\,\mathrm{B})^x$.

When, with respect to the base a, x is any logarithm of y of the $\grave{\imath}$th order, the $\grave{\imath}$th value of a^x will $= y$.

Employing the mode of expression above explained, I conceive that the chief novelty of my system consists, not in showing that any assigned quantity, relatively to a given base, has an infinite number of logarithms (which was known before), but in showing that it has an infinite number of orders of logarithms, and an infinite number of logarithms in each order.

Thus, all the Neperian logarithms of 1 have been hitherto supposed to be comprised in the formula

$$\sqrt{-1}\,2i\pi$$

whereas [32], on supposing R = 1, S = 0, A = e, and B = 0, gives the more general formula

$$\frac{2i\pi}{2i\pi - \sqrt{-1}}$$ [Note K.]

A remark necessary to prevent misconception is, that, in certain cases, a logarithm may re-appear at intervals with different ranks in different orders.

NOTES.

Note A.—My knowledge of these researches is derived not from the original Essays, but from abstracts of their contents given in the Dublin Philosophical Journal, vol. ii. No. 3. p. 60. and No. 4. p. 219.

My occupations have prevented me from examining whether mathematicians have directed further attention to the extended application of the principles there promulged. In October 1826 I had obtained the results presented in this paper.

Note B.—As long as the development [18] is not illusory, its values will be independent of the value assigned at any time to the arbitrary constant c. [Vide infra, Notes E and K.]

Note C.—It is important to observe, that notwithstanding the infinite number of values of $f^{-1}a$, yet where x is a real and rational quantity, y or $f(x f^{-1} a)$ will, from the form of the function, have periodical recurrences of the same values.

Note D.—When this expression is required to assume particular values, there needs be no correspondence between the numerator and the denominator; for, y being supposed for a moment given, x, by the definition of "logarithm of y," may be any such quantity that y may be found among the values of a^x or (see [20]) $f(x f^{-1} a)$. Every value whatever of formula [22] satisfies this criterion; for, let $\frac{t-1}{t-1}\frac{y}{a}$ be any one of its values, in which the numerator and denominator are wholly independent, then will $a^{\frac{t-1}{t-1}\frac{y}{a}}$ or $f\left(\frac{t-1}{t-1}\frac{y}{a} f^{-1} a\right)$ possess among its values

$$f\left(\frac{t-1}{t-1}\frac{y}{a} t^{-1} a\right) = f f^{-1} y = y$$

Note E.—As this example seems to lead to the general consideration of diverging and illusory series, I shall endeavour to state succinctly my impressions respecting that important and delicate subject.

Instances frequently occur to the analyst of developments, in which, upon substituting a particular value for the variable in each, there is no approximation to numerical identity between the several

resulting series calculated to any number of terms, and the respective functions which they ought to represent.

Such developments have been said to be analytically accurate, notwithstanding the numerical discrepancy in each particular case. "They serve," it is argued, "to represent their functions, and by performing algebraical operations upon them, correct conclusions are attained."

Now, it appeared to me that there was some confusion of expression in asserting universally that equations were analytically true, which, numerically considered, were, in particular instances, palpably false. In ascertaining the correctness of the conclusions deduced from them, and relied upon as evidence of the truth of their premises, I observed that the formerly rejected test of numerical identity was often appealed to. Nay further, I was induced to ascribe, in the absence of other visible causes, to the intervention of such equations the limited results which were occasionally elicited where previous calculations would lead to the expectation of general ones, and even the conclusions absolutely and unlimitedly erroneous to which the mathematician was sometimes conducted by apparently undeviating paths.

To account for these difficulties, upon reverting to first principles, it will be found that the theorems of development (such as Taylor's, Maclaurin's, &c.) are based upon hypothetic reasoning to this effect, viz. "if the function be developable according to certain powers, it will be developed in a certain form," which is assigned. Now imagine a function of x, for instance, which for those values only of x that lie between certain limits, is capable of being developed according to the ascending integral powers of x, such a function, it would seem, evolved by Maclaurin's theorem, would afford an expansion which, when x transgresses those limits, would be illusory.

In the treatment of developments thus partially true, when more than one of them come in question, the extent of their compatibility should, in my opinion, be most carefully attended to; for, if two such developments of a function were equated, whereof the one was applicable for values of the variable which would render the other illusory, the consequences derived from such equation might, in proportion to the extent of those values, be partly or entirely false. An instance of the limitation introduced by the caution here recommended is to be found in Appendix § 4.

To learn how far a development was applicable, it might be useful to ascertain the error committed upon calculating n terms of the series, and, then supposing n an infinitely great integer, to observe if there were any values of the variable which would prevent the expression for the error from vanishing.

Should these reflections appear dubious or unfounded, I wish it to be fully understood that they may, in that case, be considered as operating on my results only, at most, by way of superfluous caution. Thus, if c be deemed unnecessary to the universal accuracy of the series [18], it has, at all events, the merit of ensuring its convergence.

Since writing the above, I have been informed by Professor Hamilton that M. Poisson has lately given examples of the danger of using diverging series, even when the final development to which they conduct is converging.

Note F.—This seems to prove that the logarithms of negative numbers are not in general the same as those of their positives, as Jean Bernouilli and D'Alembert thought. (See Lacroix, "Traité," &c. Introd. 82.) Hence also conversely by easy inference it seems to follow, that negative numbers have occasionally even real logarithms, contrary to the opinion that they have none whatever,

maintained in the Encyclopedia Metropolitana article Algebra, 284. Indeed, when -2 is admitted to be one of the values of $4^{\frac{1}{2}}$, the extension of the notion "logarithm" must be greatly abridged to deny that, relatively to the base 4, $\frac{1}{2}$ is a logarithm of -2.

Note G.—From this theorem it does not follow that $f^{-1}\theta^{\lambda} = \lambda f^{-1}\theta$; an expression that has only half as many values as $f^{-1}\theta + f^{-1}\theta$, which admits the addition of any one value of $f^{-1}\theta$ to any other.

This instance is adapted to give notice of a very insidious species of fallacy, whose intrusion, in reasoning on subjects like the present, should be guarded against with vigilance.

Note H.—As $2i\pi$ comprises exactly the same values as $2(i-i)\pi$, and serves as well to show that the integer in the numerator of [26] may be chosen without reference to that in the denominator, it is preferred for briefness and concinnity in a general formula.

Note I.—The solution of this problem assists in constructing the figure whose equation is $x^{A} + \sqrt{-1}^{B} = y$.

M. Vincent has inserted in the commencement of the 15th volume of the "Annales de Mathématiques," &c. published at Nismes in 1824 and 1825, and edited by M. J. D. Gergonne, an ingenious paper on the construction of some discontinuous transcendental curves. His paper is entitled "Considerations nouvelles sur la nature des courbes logarithmiques et exponentielles. Par M. Vincent, Professeur de Mathématiques au Collège Royal de Reims, ancien élève de l'école normale." His general principles appear to me to be correct; but, in my opinion, he has occasionally fallen into error. For instance, he seems to take it for granted when a is positive, that whatever value of a^x be considered, $\mathrm{d}\,a^x = \mathrm{l}\,a\,a^x\,\mathrm{d}\,x$; whereas, when the i^{th} value of a^x is considered (see Appendix § 7.) $\mathrm{d}\,a^x = (\sqrt{-1}\,2\,i\pi + \mathrm{l}\,a)\,a^x\,\mathrm{d}\,x$.

To obviate some objections to my general theory, I may here observe incidentally that M. Stein, who has occasionally written on the subject of logarithms in the same journal, would introduce a very confused and inconvenient notation by supposing a^x to vary its signification according to the form in which the value of x is expressed—by supposing, for instance, that, while $a^1 = a$, $a^{\frac{1}{2}}$ would $= (a^2)^{\frac{1}{4}}$ or $\pm a$. Hence, by the same analogy $a^{\frac{\sqrt{2}}{\sqrt{2}}}$ would $= a \,.\, 1^{\frac{1}{\sqrt{2}}}$. According to the usual interpretation of a^x, which I have adopted, and by which it is identical with $f(x\,f^{-1}a)$, a^1, $a^{\frac{1}{2}}$ and $a^{\frac{\sqrt{2}}{\sqrt{2}}}$ have all the same signification.

The following definition of a^x, derived from the characteristic property which led to the extension of the exponential notation beyond integral exponents, has been suggested to me by my friend Mr. Hamilton, Royal Astronomer of Ireland:

"a^x comprises every successive function φx of x, which, independently of x and y, satisfies the conditions $\varphi x\,\varphi y = \varphi(x+y)$ $\varphi 1 = a$."

From this definition does not follow, in all its generality, the equation $a^x\,a^y = a^{x+y}$, for the product of the i^{th} value of a^x (which I would designate by $a_i^{\,x}$) multiplied by the i^{th} value of a^y is not necessarily among the values of a^{x+y}; a legitimate consequence of the definition of a^x is the equation $a_i^{\,x}\,a_i^{\,y} = a_i^{\,x+y}$.

Note K.—To exemplify the agreement with which the positions we have established lead by different processes to the same conclusion, it may be mentioned that the same general formula for the Neperian logarithms of 1 would be obtained from [23], on supposing $y=1$, $c=0$, $a=e$ and $c=\sqrt{-1}$, or, more concisely, from [26], on supposing $K^0=1$.

If, however, in [23] we had selected other values for c and c, consistently with the convergence of the numerator and denominator; *e. g.* if c were supposed $=2i\pi$ and $c=2i\pi+\sqrt{-1}$, upon making all the necessary substitutions; formula [23] would produce

$$\frac{2i\pi-2i\pi}{2i\pi-2i\pi-\sqrt{-1}}$$

Now though this formula has precisely the same values as $\frac{2i\pi}{2i\pi-\sqrt{-1}}$, yet their arrangement is different. In general, therefore, [23], from its liability to alter the arrangement of its values by the alterations imparted to c and c, cannot be resorted to for the definitive computation of the orders and ranks of logarithms. It was from the necessity of establishing a standard (whose only requisite is that, when once determined, it should not be varied,) from which to commence such computation, that, in Appendix, § 5, I fixed arbitrarily (the consideration of superior simplicity abstracted) on that value of $\cos^{-1}\frac{R}{\sqrt{R^2+S^2}}$ which I denote by $\grave{\cos}^{-1}\frac{R}{\sqrt{R^2+S^2}}$, although any other defined value $\grave{\cos}^{-1}\frac{R}{\sqrt{R^2+S^2}}$, which would satisfy the equation

$$f\grave{\cos}^{-1}\frac{R}{\sqrt{R^2+S^2}}=\frac{R+\sqrt{-1}S}{\sqrt{R^2+S^2}}$$

would have answered the same purpose.

When R is negative and $S=0$, according as we decide to consider 0 positive or negative, $\grave{\cos}^{-1}\frac{R}{\sqrt{R^2+S^2}}$ will $=$ either $+\pi$ or $-\pi$; in every other case the value of $\grave{\cos}^{-1}\frac{R}{\sqrt{R^2+S^2}}$ will be definitively fixed by [29].

If $\overset{0}{l}a$ designate the 0th Neperian logarithm of a of the 0th order, [32] may be expressed as follows:

$$x=\frac{\sqrt{-1}2i\pi+\overset{0}{l}y}{\sqrt{-1}2i\pi+\overset{0}{l}a}$$

which may be compared with (3) and (4).

XVII. *On the reflection and decomposition of light at the separating surfaces of media of the same and of different refractive powers**. *By* DAVID BREWSTER, *L.L.D. F.R.S. L. & E.*

Read February 12, 1829.

IT is a necessary result of the Newtonian theory of light, and one which NEWTON himself deduced, that when white light is incident on the separating surfaces of different media, it preserves its whiteness after reflection, excepting in those cases where the thickness of one of the media is beneath the 80 millionth part of an inch.

When the discovery of the different dispersive powers of bodies was made, it should have been obvious that reflected light never could be perfectly white under any circumstances, though such a modification was not likely to be detected in the usual routine of optical experiments. The only philosopher indeed who, in as far as I know, has made any experiments on the subject is Mr. HERSCHEL; and as his opinions may be considered as representing those of the present period, I shall make no apology for quoting them.

"The phenomena which take place when light is reflected at the common surface of two media are such as from the above theory we might be led to expect, with the addition however of some circumstances, which lead us to limit the generality of our assumptions, and tend to establish a relation between the attractive and repulsive forces to which the refraction and reflection of light are supposed to be owing. For it is found that when two media are placed in perfect contact, (such as that of a fluid with a solid, or of two fluids

* The principal experiments contained in this paper were made in 1816, and were signed by the president of the Physical Class of the Royal Society of Edinburgh. A brief notice of them was published in the Quarterly Journal for July—October, 1816, and a more extended paper was read at the Royal Society of Edinburgh on the 4th of January 1819. The difficulties of the subject, however, prevented me from pursuing it but at distant intervals; and the more fertile topic of polarisation afterwards required all the time I could devote to such inquiries.

with one another,) the intensity of reflection at their common surface is always less the nearer the refractive indices of the media approach to equality; and when they are exactly equal, reflection ceases altogether, and the ray pursues its course in the second medium, unchanged either in direction, velocity, or intensity. It is evident from this fact, which is general, that the reflective or refractive forces, in all media of equal refractive densities follow exactly the same laws, and are similarly related to one another; and that in media unequally refractive, the relation between the reflecting and refracting forces is not arbitrary, but that the one is dependent on the other, and increases and diminishes with it. This remarkable circumstance renders the supposition of the identity of form of the function expressing the law of action of the molecules of all bodies on light indifferently, less improbable.

"To show experimentally the phenomena in question, take a glass prism or thin wedge of a very small refracting angle (half a degree for instance: almost any fragment of plate glass indeed will do, as it is seldom the two sides are parallel), and placing it conveniently with the eye close to it, view the image of a candle reflected from the exterior of the face next the eye. This will be seen accompanied at a little distance by another image reflected internally from the other face, and the two images will be nearly of equal brightness, if the incidence be not very great. Now apply a little water, or a wet finger, or still better, any black substance wetted, to the posterior face, at the spot where the internal reflection takes place, and the second image will immediately lose great part of its brightness. If olive oil be applied instead of water, the defalcation of light will be much greater; and if the substance applied be pitch, softened by heat so as to make it adhere, the second image will be totally obliterated. On the other hand, if we apply substances of a higher refractive power than glass, the second image again appears. Thus with oil of cassia it is considerably bright. With sulphur it cannot be distinguished from that reflected at the first surface; and if we apply mercury or amalgam (as in a silvered looking-glass), the reflection at the common surface of the glass and metal is much more vivid than that reflected from the glass alone. The destruction of reflection at the common surface of two media of equal refractive powers explains many curious phenomena, &c."*

* Treatise on Light, § 547, 548.

In the year 1814, when I was investigating the law of polarisation for light reflected at the separating surface of different media*, I had occasion to inclose oil of cassia between two flint glass prisms. The blue colour of the reflected light at first surprised me; but though the fact was new, and the experiment itself interesting, the decomposition of the light was obviously explicable upon known principles. Although the refractive density of oil of cassia exceeds greatly that of flint glass for the mean rays, yet the action of the two bodies on the less refrangible rays is nearly the same; and hence the red rays must be in a great measure transmitted, while there will be reflected a small portion of the orange, a greater portion of the yellow, a still greater proportion of the green, and a very great proportion of the blue: and consequently the colour of the pencil formed by reflection must necessarily be principally blue.

By using different kinds of glass and different oils I obtained various analogous results, in which different rays of the spectrum were extinguished by effecting (as far as possible) an equilibrium between the two opposite actions exerted upon them by the solid and the fluid media. When the blue light is extinguished, the colour of the reflected pencil has a yellow tinge; and it is obvious that the resulting pencil can never have a decided colour, but must always be bluish or yellowish.

As the indices of refraction remain the same for all obliquities of incidence, the tint of the reflected pencil, though it varies in intensity, can never vary in its colour; so that we cannot obtain any succession of tints or coloured rings from this partial decomposition of the incident rays.

These observations establish it as a general fact, that in all cases of reflection from transparent surfaces, the reflected pencil must necessarily have a different tint from the incident pencil, excepting in the extreme case where the two bodies in contact have mathematically the same refractive and dispersive powers.

I was now anxious to observe the effect of an approximation to this last condition, or to a perfect equilibrium of all the forces which affect the incident rays; as it is often in extreme cases, and at a limit such as this, that nature delights in the development of new phænomena. This experiment, however, was attended with more difficulty than I expected; but amid the numerous

* Phil. Trans. 1815, p. 137.

disappointments which it occasioned, I was led to the results which I shall now proceed to describe.

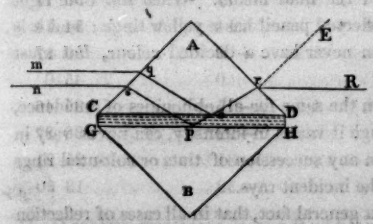
The solids which I employed were two prisms of plate glass, which I shall call A and B. The prism A, whose section was an isosceles right-angled triangle, had its base polished at the plate glass manufactory where it was made. The prism B was executed for me by Dollond, and very finely polished, having also its section a right-angled isosceles triangle. The refractive indices were

$$\text{In A} \ldots m = 1.508$$
$$\text{In B} \ldots m = 1.510$$

The fluids which I employed were castor oil and balsam of capivi, the latter having a greater and the former a less refractive power than the glass prisms. The refractive indices were

$$\text{In castor oil} \ldots\ldots\ldots m = 1.490$$
$$\text{In balsam of capivi} \ldots m = 1.528$$

Fig. 1.



The prisms A, B were now fixed together as in fig. 1, and a film C D of castor oil interposed between them. A ray of light R r will after refraction at r be reflected in the direction $o\,q\,m$ from the surface C o D which separates the prism A and the oil; and another portion of it will be reflected in the direction $p\,s\,m$ from the surface G p H which separates the prism B and the oil. In order that the two rays $q\,m$, $s\,n$ may be sufficiently separated, the common sections of the faces which contain the right angle are slightly inclined to each other.

When the angle of incidence R r E is very great, the light suffers total reflection at the surface C o D. Within the limit of total reflection the light $o\,q\,m$ is yellow; and by diminishing the angle of incidence gradually, the pencil $o\,q\,m$ passes through all the tints of nearly three orders of colours, as shown in the following Table.

	Colours.	Angles of Incidence R r E.	Angles of Incidence on Surface C e D*.
1st Order.	Yellow	70°	88° 33′
	Orange	63	81 13
	Red	61	80 27
	Pink	59½	79 51
	Limit of pink and blue	58	79 14
2nd Order.	Bluish pink	57	78 46
	Full blue	55	77 54
	Greenish blue	52	76 30
	Yellowish blue	48	74 32
	Yellow	41	70 46
	Reddish yellow	34	66 46
	Redder still	26	61 54
	Red	21	59 4
	Pink red	17	56 11
	Limit of pink and blue	14	54 14
3rd Order.	Blue	+ 9	50 57
	Bluish green	0	45 0
	Yellowish	−15	35 46
	Full yellow	−22	30 37
	Reddish yellow	−31	25 21
	Pink	−52	13 30

The colour of the pencil $p s n$ produced by the other separating surface $G p H$ is at all incidences a faint yellowish gray, (which is best seen by turning the system of prisms upside down; and receiving the ray $R r$ upon the prism B, so that the reflected ray $p s n$ may not pass through the oil;) and its intensity suffers very little change. This fact is a very remarkable one, and arises (as will be presently seen) from some specific property of the glass itself. When the lower prism is of the same glass as A, and produces the colours in the preceding table at different angles of incidence from those of A, the play of colours

* This column is calculated from the formula $A = 45° \pm \frac{\sin. I}{m}$, I being the angles of incidence in the 1st column, A the angles in the 2nd, and $m = 1.508$ the refractive index of the glass.

is particularly fine, and the whole phænomenon is one of the most beautiful in physical optics.

When the incident light is homogeneous, no colours of course are seen; but the reflected pencils have their maxima and minima of intensity, like the rings of thin plates or the fringes of inflected light when formed by homogeneous rays.

The following are the periods for red and for blue light.

	Red Light.	Blue Light.
1st minimum . . .	77° 54′	. . . 80° 27′
2nd minimum . . .	50 57	. . . 59 4

If we substitute for the prism A a square prism, the tints are thrown more closely together; and if the luminous object is a long stripe of bright light, we may see most of the colours at one view.

If we now apply heat to the oil so as to diminish its refractive power, the brightness of the colours is greatly diminished, and the first period is completed at a less angle of incidence.

Such are the phænomena which take place when the refractive power of the glass exceeds that of the fluid. We shall now see what happens when the fluid has a greater refractive energy than the solid; a case of peculiar interest, because we are able to reduce the two refractive powers to a perfect equality for any given ray of the spectrum.

The same prisms being employed, let the film C D H G be now balsam of capivi. Before total reflection takes place, the reflected pencil is perfectly white: it then becomes yellow, and passes through the same orders of colours as in castor oil. All the colours, however, are produced at less angles of incidence, the 1st order terminating at an angle of 64° 58′, as appears from the following Table, in which I have given only the leading tints.

	Colours.	Angles of Incidence R r E.	Angles of Incidence on the Surface C o D.
1st Order.	Yellowish	47°	74° 10′
	Yellow	41	70 47
	Pink red	36	67 57
	Pink	33	66 10
	Limit of pink and blue	31	64 58

	Colours.	Angles of Incidence R ▸ E.	Angles of Incidence on the Surface C o D.
		°	° ′
2nd Order.	Bluish pink	28	63 8
	Full blue	26	61 54
	Bluish green	22	59 23
	Bluish yellow	18	56 50
	Yellow	10	51 37
	Reddish yellow	1	45 40
	Red	— 8	39 42
	Pink red	—13	36 25
	Limit of pink and blue	—16	34 28
3rd Order.	Blue	—22	30 37
	Bluish green	—26	28 56
	Green	—30	25 29
	Yellowish green	—41	19 13

Having ascertained that at a temperature of about 94° the mean refractive index of the balsam was nearly equal to that of the glass prisms, I proceeded to examine the influence which a varying temperature from 50° to above 94° exercised over the intensity and the colour of the reflected pencil.

The prisms were therefore fixed so as to exhibit the full blue of the second order, and the heat was gradually applied. The colour of the tint was obviously improved by heat, though the intensity of its light was diminished. No particular change marked the instant when the refractive density of the glass and the balsam was equal. Beyond 94° the intensity of the tints increased in consequence of the diminution in the refractive power of the balsam; but when the temperature was considerably augmented, the tints completely disappeared.

Let us now attend to a very remarkable phænomenon exhibited in the relative intensities of the pencils *o q m* and *p s n*. At an angle of incidence of 61° 54′ on the surface C o D, and at a temperature of about 50°, the pencil *o q m* is a full blue, while *p s n* is a grayish white of rather less intensity than the blue pencil. By increasing the angle of incidence, the pencil *o q m* increases rapidly in intensity, while the gray pencil diminishes slowly: so that at an incidence of 74°

o q m is ten or twelve times more luminous than *p s n*; whereas at smaller incidences than 61° 54′, the pencil *p s n* surpasses *o q m* in the intensity of its light. By the application of heat *p s n* becomes yellowish-white, and increases greatly in intensity. It now approaches at oblique incidences to the brightness of *o p m*, but is still inferior to it, while at small incidences it surpasses it in intensity.

In the preceding experiments the solid had nearly the same refractive density as the balsam. We shall now take a solid, namely obsidian, which has nearly the same refractive power as the oil.

When the lower prism B is obsidian, and the film C D, H G balsam of capivi, the ray *p s n* passes through three orders of colours; namely,

1st Order.	White,
	Yellow,
	Red,
	Limit of red and blue at 73°.
2nd Order.	Blue,
	Bluish-green,
	Yellowish white,
	Reddish white,
	Pink, faint.
3rd Order.	Bluish,
	Bluish-white.

These colours are by no means good, nor are they much improved by heat, which approximates the refractive power of the fluid to that of the solid. The heat reduces the orders to two, each colour being now developed at a much smaller angle of incidence. The first order, for example, which ended at an incidence of 73°, now ends at an incidence of 52°. When the heat is so great that we cannot touch the prisms with the hand, all the colours are effaced.

If we now substitute the castor oil in place of the balsam, no colours are visible; but the reflected pencil *p s n* is white and bright, notwithstanding the coincidence between the refractive energies of the solid and the fluid. Heat increases the intensity of the pencil, but produces no colour.

Hitherto we have considered the action of the two surfaces of the film as exhibited separately in the two images displaced laterally by the prismatic shape of the fluid. We shall now briefly notice the phænomena which are pre-

sented by the superposed images when the film of fluid has its surfaces parallel. If the two prisms A, B give separately the same periods of colours, but at different angles of incidence, then the resulting tints are very irregular and indistinct; but if the maxima of the periods produced by one prism coincide with the minima of the periods produced by the other, the colours will be almost wholly obliterated, though it is not easy to ensure the condition on which this compensation depends. When the separate prisms give exactly the same periods at the same angles of incidence, then the minima of the one will correspond with the minima of the other, and the maxima with the maxima; so that the combination produces the same periods of colours that were produced by each prism separately; but the intensity of the tints is doubled. This duplication of the tints is easily observed by bisecting a prism which produces distinct periods, and separating the two halves by a fluid film.

Although the preceding experiments are sufficient to establish the existence, and explain the nature of this class of phænomena, yet, as they will probably lead to very important consequences in the theory of light, I shall make no apology for giving an account of another series, of a very instructive kind, and performed with fluids particularly fitted for the investigation. I continued to use the same prisms of plate glass; but as the oil and balsam formerly employed differed considerably in refractive power from the glass, I sought for two oils with nearly the same mean refraction as the prisms; and those which I selected were oil of cummin and distilled wood oil*, which were fortunately capable of being mixed together with great facility. Their refractive powers for the mean yellow rays were nearly as follows:

	Indices of Refraction.
Oil of cummin	1.512
Plate glass, prism B	1.510
Oil of cummin and wood oil mixed	1.5085
Plate glass, prism A	1.508
Wood oil	1.506

As nothing depends on the numerical accuracy of these indices, I did not measure them with any peculiar attention; but by immersing a right angle

* This oil was sent to me from the East Indies by George Swinton, Esq., Secretary to the Government at Calcutta.

of each prism in a vessel containing each of the three oils, I carefully determined that, at a temperature of 50°, they acted on the homogeneous yellow light of a monochromatic lamp, in the order in which they are above placed.

I now combined each of the oils in succession with the two prisms, as shown in Fig. 1, and in all the combinations the separating surface of the prism A and the oils produced from a white flame, nearly three orders of colours of the same intensity, and nearly at the same angles of incidence, as in balsam of capivi; while the separating surface of the prism B and the oils reflected only a faint gray image of very little intensity, and generally growing fainter as the angle of incidence increased.

When the homogeneous yellow light of a monochromatic lamp was used, the separating surface of the prism A and all the oils produced the first minimum at nearly the same angle of incidence; and though I applied heat gradually to the least refractive oil, and cold to the most refractive one, so as to produce a perfect compensation of opposite refractions for the yellow rays, yet no perceptible change appeared either in the place of the first minimum or in the intensity of the reflected light. In the case of the mixed oil the compensation was effected without any other change of temperature but what was occasioned by a change of position in the apartment.

In the expectation of discovering some solid or fluid medium which would produce with plate glass a greater number of orders of colours, I made the experiments contained in the following Tables.

TABLE, Showing the periods of colours produced at the separating surfaces of plate glass and oils and other fluids.

Names of Oils.	Image at the Surface of Prism A.		Image at the Surface of Prism B.
Oil of Cassia	Pale red tints at 65° of incidence; then at less incidences pale blue, and then pale red.	Heat strengthens the tints a little.	White and bright.
Balsam of Peru ..	Slight tinges of red; blue as above.	Two faint orders of colours brought out by heat.	Yellowish white.
Oil of Anise-seeds	The tinges of two orders of colours. Limit of pink and blue of the first order at an incidence less than 65°.	Heat of 200° brings out two good orders of colours.	Grayish or bluish white.
Balsam of Styrax..	Tinges of two orders of colours.	Improved by heat.	Bright white.

Names of Oils.	Image at the Surface of Prism A.	Image at the Surface of Prism B.
Canada Balsam ..	Above two orders of colours; pink of second the best. Improved by heat.	Grayish or bluish white.
Oil of Tobacco ..	Two faint orders of colours. Heat brings out nearly three.	Grayish white.
Oil of Cloves	Two faint orders. Heat brings out part of a third. First limit of pink and blue about 65° of incidence.	Yellowish white but bluish gray with heat.
Oil of Sassafras ..	Two orders. First red pale. First blue good.	Grayish white.
Balsam of Capivi ..	See page 191.	
Muriate of Antimony	Two tolerably distinct orders of colours.	Grayish white.
Oil of Cummin....	Two beautiful orders. A fine yellow in the second order. Heat spoils them all.	Faint grayish, becoming more intense and yellow by heat.
Nut Oil	Two faint orders, the second red and second blue being tolerably good. Heat brings out two fine orders, the first limit of pink and blue ending at about 76° of incidence.	Yellowish white.
Oil of Pimento....	Three good orders of colours. First limit of pink and blue at 65° of incidence.	Pale blue, very faint at great incidences.
Oil of Sweet Fennel-seeds.....	Two orders; pink good.	Bluish gray.
Wood Oil.....	Three good orders of colours. First pink and blue fine. First limit of pink and blue ends at 65°.	Bluish gray, weaker at great incidences.
Oil of Amber	Two excellent orders of colours. First limit of pink and blue at 65°. Improved by heat.	Pale blue, very faint at great incidences.
Oil of Rhodium ..	Two and a half good periods. First limit at 65°. Heat injures them.	Yellowish white.
Treacle.....	At temp. 50° three orders, which are not good, especially the pink of first and blue of second order. Heat brings out three splendid orders with periods, as in castor oil *.	Yellowish white.
Balsam of Sulphur .	Three fine orders. First limit of pink and blue at about 67°.	Faint gray, getting fainter and bluer at great incidences.
Honey	Two pretty good orders. First limit at about 65°.	Slightly yellowish white.
Oil of Angelica ..	Two and a half orders. First pink and first blue fine; second red good.	Whitish yellow.
Oil of Nutmeg....	Three not very bright orders. First limit at 78°.	Whitish yellow.

* The treacle used in this experiment is much inferior in refractive power to the prism A.

Names of Oils.	Image at the Surface of Prism A.	Image at the Surface of Prism B.
Oil of Marjoram ..	At a low temperature the orders are scarcely perceptible, the second limit only being visible. Heat brings out the second limit at a less incidence, and creates the first limit at 79°.	Whitish yellow.
Castor Oil	See page 193.	
Oil of Hyssop	Colours very faint. Heat brings out three good orders. First limit at 77°.	Whitish yellow.
Oil of Fenugreek..	Colours rather better than the preceding. Heat brings out three good orders. First limit at 75°.	Whitish yellow.
Oil of Caraway-seeds	Two orders, not good.	Whitish yellow.
Oil of Thyme	Slight tinges of colour. Heat brings out two good orders.	Yellowish white.
Oil of Turpentine .	Two tolerably good orders. First limit at 74°.	Whitish yellow.
Cajeput Oil	Two tolerably good orders. First red bad, second red good.	Yellowish white.
Linseed Oil	Two extremely faint orders. Three good orders brought out by heat. First limit at 75°.	Yellowish white.
Train Oil	Three very good orders. First red and first blue excellent. First limit at 73°. Heat spoils the first order.	Yellowish white.
Oil of Savine	Almost no colours, both images being yellowish, and that of B brightest. Heat brings out three orders. First limit at 80°, which a greater heat brings to 75°.	Yellowish white.
Oil of Pennyroyal .	Almost no colours. A sort of bluish gray when cold. Heat brings out two good orders when temp. only 90°, but greater heat injures them.	Yellowish white.
Oil of Almonds ..	Three tolerable orders. First red bad, second red good.	Yellowish white.
Oil of Mace	Gives three and a quarter orders when cold. First limit at about 80°. When the film of the oil begins to crystallize, it displays red, blue, and greenish tints, at the same incidence, in different places.	Pretty bright.
Oil of Spearmint ..	Very faint colours. Heat brings out three good orders. First limit at about 77°.	Yellowish at great incidences.
Oil of Lemons	Three fine orders. First limit at 74°. Heat destroys the first order.	Yellowish white.
Oil of Dill Seed ..	Two poor orders of colours. First limit at 73°. Heat improves them.	Yellowish white.
Oil of Peppermint .	Two good orders. First limit 73°. Heat destroys the first order.	Yellowish white.
Oil of Rapeseed ..	Two very faint orders. First limit at 65° when improved by heat.	Bluish gray.
Naphtha from Persia	Three very good orders.	White.

Names of Oils.	Image at the Surface of Prism A.	Image at the Surface of Prism B.
Oil of Bergamot ..	Three very fine orders. First limit at 73°. Heat spoils first order.	Yellowish white.
Oil of Beech Nut	Three excellent orders, and well defined. First limit at 73°. Heat spoils first order.	Yellowish white.
Spermaceti Oil....	Two tolerable orders. First red and blue bad, second red and blue good. First limit at 73°.	Yellowish white.
Oil of Olives	Three good orders. First limit at 73°.	Whitish yellow.
Grass Oil.....	Three good orders. First limit at 73°.	Grayish white.
Oil of Rosemary ..	Two good orders and more. First limit at 73°.	Whitish yellow.
Oil of Poppy	Three excellent orders. First limit at 73°. Heat injures the colours.	Yellowish white.
Oil of Lavender ..	Three good orders. First red and first blue very fine. First limit at 74°.	Yellowish white.
Oil of Camomile ..	Two good periods. First limit about 60°.	Bright yellowish white.
Oil of Wormwood .	Three good periods. First limit at 71°, but not well defined.	Yellowish white.
Bhela Juice	Three faint orders at low temperatures, but finely brought out by heat. First limit at 73°.	Yellowish white.
Muriatic Acid	Traces of tints.	Yellowish white.
Sulphuric Acid....	Two pretty good orders.	Yellowish white.
Vitreous Humour of the Haddock	Traces of colours.	Bright.
Oil of Rhue	No colours.	Bright.
Oil of Boxwood ..	No colours.	Bright.
Alcohol	Traces of reddish, bluish, and greenish yellow tints.	Bright.
Water.....	Traces of tints.	Bright.

The experiments * recorded in the preceding pages may be divided into two classes.

I. Those which establish the existence of reflecting forces at the confines of media of the same refractive power; and,

* These experiments have been extended to a great number of mixed oils and to soft solids, gums and resins, combined with the prisms A and B. I have also substituted for these prisms others of different kinds of glass, which give similar results; and I have examined the phænomena at the confines of different fluids and a great number of minerals of various refractive powers between chromate of lead and fluor spar.

II. Those in which periodical colours are produced at the confines of particular kinds of glass, and various fluids and soft solids.

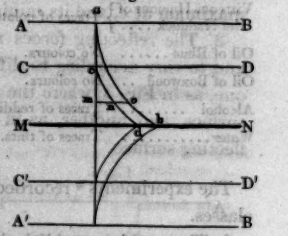
From the first of these classes of facts the following conclusions may be drawn.

1. The reflective and refractive forces in media of the same refractive power do not follow the same law. This result is clearly established by the experiments with the prism B, which produced no orders of colours. Not only was there a strong reflected pencil when a perfect equilibrium was effected between the opposite refracting forces, but there was not even an approximation to evanescence as the forces advanced to their point of compensation. The same result was obtained with a prism newly ground and polished.

2. The force which produces reflection varies according to a different law in different bodies. If the curve which represents the law of the reflective force were exactly the same in the prism B and the fluids combined with it, then the ordinates which represent the intensity of the force at any given point would be exactly equal, and consequently there would be a perfect equilibrium of opposite actions, and no reflection of the passing light. But as a copious reflection takes place even when the opposite forces are balanced, we are entitled to infer that the law of the two forces is different.

The reflective forces in the solid and fluid may be conceived to decrease in various ways.

Fig. 2.



1. They may extend to different distances from the reflecting surface, and decrease according to the same law. This relation is shown in Fig. 2, where M N is the reflecting surface, A B the limit of the sphere of reflecting activity in the solid, and C D that in the fluid,—*a o b* the curve which represents the reflecting force of the solid, and *c n d* that of the fluid. In this case there can be no compensation of opposite reflections, and an unbalanced reflecting force will exist at almost every point of the sphere of reflecting activity. From *a* to *c* the light will be acted upon by the undiminished force of the solid. At *c* the force of the fluid begins to oppose that of the solid, and the unbalanced force at any other line *m o* is equal to *n o*, the difference of the two forces *m n*, *m o*. In this case there will be a sphere of reflecting activity extending from A B to A′ B′, and such a combination must reflect light without refracting it.

2. The reflecting forces may extend to different distances, and vary according to a different law. Two cases of this kind are shown in Fig. 3 and 4.

Fig. 3.

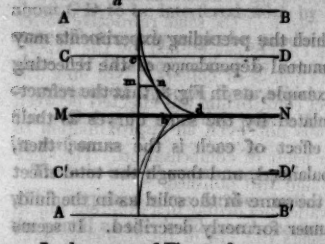
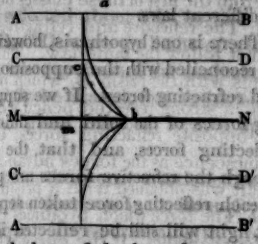


Fig. 4.



In the case of Fig. 3. the curves expressing the law of the forces have a common ordinate $m\,n$ where the reflections are compensated; but from a to n the reflecting force of the solid will predominate over that of the fluid, and from n to d the force of the fluid will predominate over that of the solid; so that in such a combination there will be two spheres of reflecting activity, one of which begins where the other ends.

In the case of Fig. 4, where the curves have the same maximum ordinate $m\,b$, we shall have a sphere of reflecting activity commencing at a, reaching its maximum at c, and its minimum at b.

3. The reflecting forces may be conceived to extend to the same distance, and to vary according to different laws. Two cases of this kind may occur; one, as in Fig. 5, where the maximum of unbalanced force is distant from the surface, and another, as in fig. 6. where the maximum takes place at the reflecting surface.

Fig. 5.

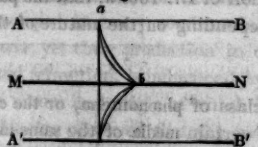
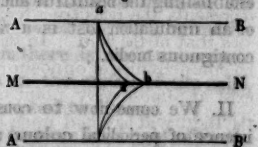


Fig. 6.



In the conclusions which we have drawn respecting the independence of the reflecting and refracting forces, it was supposed that the latter follow the same

law in solids and fluids. There seems to be no method of determining whether or not this is the case; for experiment indicates only the total effect, or the sum of all the ordinates, and these may be compensated, though they vary according to different laws.

There is one hypothesis, however, on which the preceding experiments may be reconciled with the supposition of the mutual dependence of the reflecting and refracting forces. If we suppose, for example, as in Fig. 3, that the refracting forces of the solid and fluid are regulated by the same curves as their reflecting forces, and that the absolute effect of each is the same; then, though the refractive forces are perfectly balanced, and though the total effect of each reflecting force taken separately is the same in the solid as in the fluid, yet light will still be reflected in the manner formerly described. It seems highly probable that the law of the refracting force varies in different bodies; and if we take for granted the mutual dependence of the refracting and reflecting forces, the preceding experiments will establish a variation in the law of the refracting forces of different media.

In the undulatory system, the preceding facts may be explained by supposing that the density or elasticity of the ether varies near the surface of different bodies; a supposition in itself highly probable, and which has been already adopted to explain the loss of part of an undulation in several of the phænomena of interference. In such a case the reflection of the light will commence at a line where the density or elasticity of the ether in the first medium begins to change, and will continue till the ray has penetrated to that part of the second medium where the density or elasticity of the ether is uniform. In this theory, therefore, the preceding facts may be regarded as proving the variable condition of the ether near the surfaces of bodies, and of establishing the beautiful and sagacious deduction of Dr. Young, that the part of an undulation lost is a variable fraction depending on the nature of the contiguous media.

II. We come now to consider the second class of phænomena, or the existence of periodical colours at the confines of certain media of the same and of different refractive powers.

That the periods of colour arise, as in all similar phænomena, from the inter-

ference of two portions of light cannot be questioned; though it does not appear how these interfering pencils are generated. If we adopt the hypothesis of the reflecting forces shown in Fig. 4, we may conceive the light reflected about C D to be interfered with by the light reflected about C′ D′, so that the same effect nearly might be produced as if C D, C′ D′ were the limits of a thin plate. If this supposition is not admissible, we may hazard the conjecture, countenanced by some facts which will presently be stated, that an invisible film differing in refractive power from the plate glass, has been formed upon its surface.

There is one phænomenon which has been more than once mentioned, and which requires some further notice; namely, the decrease in the intensity of the pencil as the incidence becomes more oblique. In re-examining this very perplexing fact, which takes place in the prism B though it does not produce periodical colours, I have observed at a great incidence a distinct change of colour, from a bluish gray to a blue; so that I have no doubt that in this case the tints are those of a long period approaching slowly to its minimum. This consideration led me to suppose that in the case of balsam of capivi and other fluids, where the first order ends at and below 65°, there might be another minimum between that angle and 90°, which was prevented from showing itself by the intensity of the reflected light. This conjecture was confirmed by a careful repetition of the experiment with cubes of glass, and also by another prism in which the only tint was a pink red at an incidence of about 85°, and a blue shading off into a greenish gray at less angles of incidence. In this case, then, there was only one minimum at about 85°. A slight diminution of temperature shifted this minimum towards 90°, while an increase of temperature brought it to a lesser incidence than 85°.

Although there can be little doubt that periodical tints are more or less developed in every combination of solids and fluids of the same refractive power, yet their production in combinations where there is much uncompensated refraction, is influenced by certain changes on the surface of the solid, the nature and origin of which I have in vain attempted to discover.

Having observed that the colours occasionally became less bright after the media had remained some time in contact, and that different parts of the same surface produced the same tint at inclinations sensibly different, I took a prism which gave with castor oil three fine periods; and having brought it to a white

heat, I then ground and repolished its faces. It now ceased to give the same periods as before; but it still decomposed the white light reflected from its confines with balsam of capivi, and reflected a strong pencil of a blue colour, even when the opposite refractions were perfectly compensated. I now ground and repolished one of the faces of the obsidian already mentioned. It also ceased to give the colours with balsam of capivi formerly described; but it now produced, when combined with castor oil, with which it previously gave no colours, a beautiful yellow pencil, the reflected light being white at great incidences, and becoming yellower as the ray approached the perpendicular. In order to ascertain what changes might be owing to the processes of grinding and polishing, I sought out an old face of fracture in a plate of glass, whose wrought surfaces gave fine periodical colours; and I formed a new face of fracture. The old face which had been exposed for ten years gave the usual orders of colours; but the new face gave only one colour, which was a bright blue, but which, from the nature of the surface, I could not trace to high or low incidences.

As these results seemed to indicate that the glass had received from exposure to the air some incrustation, or had absorbed to a small depth some transparent matter in a minute state of division, or had suffered some change in its mechanical condition, I made various fruitless attempts to ascertain the nature of the change. No superficial tarnish could be rendered visible, either by the microscope or by any other means. I boiled the prisms in muriatic acid, and in strong alkaline solutions: I steeped them in alcohol, and applied a strong pressure along their surfaces; but I could not in the slightest degree change their action upon light.

If a superficial film had been formed upon the glass of such a thickness as to give the periodical colours, then its refractive power must be different from that of the glass. I therefore took a prism which gave the periodical colours, and another of the same glass which had been deprived of this property; and I found that they polarised light at exactly the same angle. I then placed them upon the base of a flint glass prism with oil of cassia interposed, and I determined that the angle at which they reflected light totally was the same*. Hence it was manifest that the supposed film did not differ in refractive power

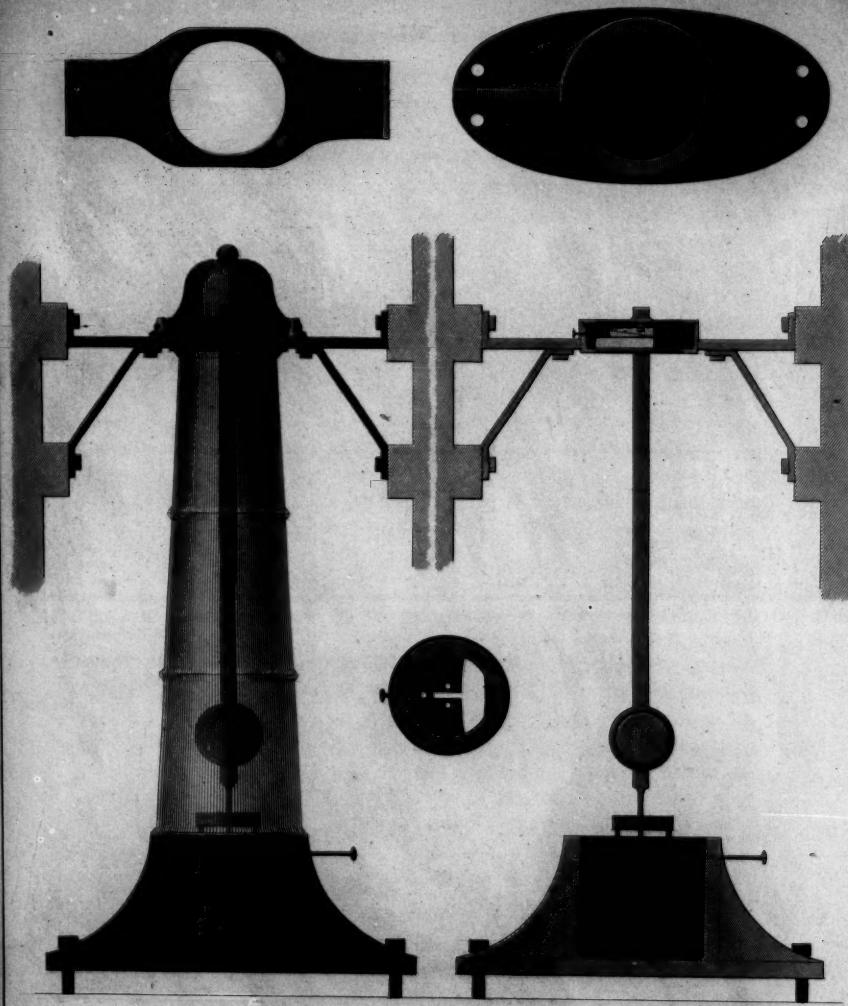
* The prism which produced the periodical colours, did not give so distinct a boundary between partial and total reflection as the other.

from the glass; and even if it did, some one of the oils with which it was in contact in the foregoing experiments must have had the same refractive energy, and must thus have deprived it of its power to develope the periodical tints. In the hope of unravelling this mystery, I took two prisms of glass cut out of the same plate, and which gave fine periodical colours with castor oil. By the aid of screws I pressed the bases of the prisms into optical contact: at great incidences the light was yellow; and by diminishing the inclination of the ray it became gradually orange and deep red when it vanished, no light being visible at smaller angles of incidence. In this experiment the surfaces of the two films, if they do exist, were brought into optical contact, so that we ought to have had orders of colours corresponding to a film of twice the thickness.

But even if such a film could be supposed to exist invisibly on the glass, it could not afford any explanation of the splendid colours which are exhibited when the solid is a crystallized mineral, and where its tint is related to its axis of double refraction. That some unrecognised physical principle is the cause of all these phænomena, will appear still more probable when I submit to the Society a paper on the very same periods of colour produced at similar angles of incidence, by the surfaces of metals and transparent solids when acting singly upon light.

The action of the surfaces of crystallized bodies presents many remarkable phænomena, in the investigation of which I have been long occupied. The results to which I have been led will form the subject of two communications. The first will treat of the action of the surfaces of bodies as an universal mineralogical character, with the description of a lithoscope for discriminating minerals. The second will contain an inquiry into the influence of the doubly refracting forces upon the ordinary forces which reflect and polarise light at the surfaces of bodies. My early experiments on this subject are recorded in the Phil. Trans. for 1819, but I have resumed the inquiry, and have obtained results of considerable interest.

Allerly, February 2nd, 1829.



Scale an inch to a foot.

P. Barlow sc.

XVIII. *On the reduction to a vacuum of the vibrations of an invariable pendulum.* *By* Captain Edward Sabine, *of the Royal Artillery, Secretary of the Royal Society.* *Communicated by* Dr. Thomas Young, *Secretary of the late Board of Longitude.*

Read March 12 and 19, 1829.

THE 128th number of Professor Schumacher's Astronomische Nachrichten, published in January 1828, contained an announcement from M. Bessel, that he had found the theory incorrect, according to which it has been customary to reduce the vibrations of a pendulum in air, to the corresponding vibrations in a vacuum; the incorrectness consisting principally, in no provision having been made in the theory, for the expenditure of a part of the moving force, on the particles of the air set in motion by the pendulum in its vibration.

On the arrival in London of the number of the Astronomische Nachrichten containing this announcement, a proposal was made to the late Board of Longitude, to submit the question, of the reduction of the vibrations to a vacuum, to the test of the most direct experiment; by the construction of an apparatus, in which a pendulum might be alternately vibrated in air of full atmospheric pressure, and in rarefied air approaching nearly to a vacuum. The expense of the proposed apparatus was estimated at 25*l.*; which sum the Board of Longitude, at the recommendation of the President of the Royal Society, and of Dr. Young Secretary of the Board, was pleased to allot for that purpose. Mr. Newman, who was employed to make the apparatus, gave great attention towards accomplishing it in the best manner; and to his care in respect to expense it is owing, that the cost has but very little exceeded the estimate. How well it has answered its intended purpose will be best collected from the experiments themselves.

The apparatus is represented in Plate VI, which may be referred to for the particular dimensions. It consists, generally, of six pieces, exclusive of the iron

frame by which the suspension is fixed securely to the wall of the apartment. The pedestal is of cast iron 2 inches thick, being a cylinder of one foot in height and a foot interior diameter, open at the top, and closed at the bottom by an horizontal plate 3 feet long by 16 inches broad, resting on four screws, by which it can be raised, depressed, and levelled. A metallic pipe communicating with an air-pump is fitted to a hole perforated at half the height of the cylinder. The metallic pipe is furnished with a stop-cock, by which the communication between the pump and the interior of the cylinder can be closed at pleasure. The three next pieces in succession above the pedestal are glass cylinders slightly conical, having their rims ground into surfaces fitting one to another. The suspension piece, which is the next above the three glasses, is a metal plate, having holes to receive the screws of the bed containing the agate planes, and to admit the pendulum to its place: it is surrounded by a circular metal ring, the outside of which forms a part of the exterior of the apparatus, and the upper and lower surfaces are ground, so as to form close joints with the glass cylinder beneath, and with a bell glass, which is the 6th piece, completing the upper part of the apparatus. The ring surrounding the suspension plate is perforated to admit a screw, which passes through a stuffing-box, and acts on an inclined plane beneath the Y's, serving to raise the pendulum on the Y's, and to lower it on the planes. The pedestal cylinder is also perforated, to admit a wire with a cross-piece at the extremity, for the purpose of stopping, or of giving fresh impulse to, the pendulum. This wire also passes through a stuffing-box.

To set up the apparatus, the pedestal is placed as nearly as can be judged in the situation it will occupy when the suspension piece is secured to the wall. A graduated arc is then fixed, by a wooden frame fitted to the interior of the cylinder, so that the arc may be seen from the coincidence telescope about 2 inches above the iron cylinder. The three glasses are then placed in succession resting on one another, and the lower one resting on the ground surface of the rim of the iron cylinder, the joints being made secure by pomatum. The foot screws of the pedestal are then adjusted, until the upper glass is brought exactly into its proper position, in regard to the iron frame by which the apparatus is ultimately to be secured to the wall. The suspension piece is then placed on the upper glass, on which it rests with its entire weight, ensu-

ring thereby the contact of the surfaces of glass and metal. The suspension piece being surmounted by the bell glass, the air is withdrawn, and the weight of the atmosphere on the exterior presses the several joints into the closest contact. Before the air is re-admitted, four screws, destined to connect the iron frame in which they work with the suspension piece, are turned until their pressure in different directions, against the outside of the ring surrounding the suspension plate, attaches it firmly to the iron frame. The frame is itself very firmly screwed to stone piers deeply imbedded in the wall on either side, and is further strengthened by brackets, fixed in the direction which is most immediately opposed to any motion of vibration, which might be communicated by the pendulum. The air is then re-admitted, the bell glass taken off, the agate planes screwed on and levelled, the pendulum suspended, with such thermometers, barometer, and gauge as may be required, and the bell glass replaced. All beneath the bell glass remains from thenceforward a fixture, the air being withdrawn and admitted at pleasure through the metallic pipe governed by the stop-cock. As the three middle glasses are pressed tightly between the suspension piece and the pedestal, neither of which can give way to their expansion, it might not be prudent perhaps to risk their fracture, by leaving them so screwed, for such a length of time as should involve a great change of temperature. To avoid this, it is only necessary to loosen the screws which connect the iron frame with the suspension piece, to tighten them again at a new temperature, and to re-level the planes.

This description applies to the apparatus as it is now established in the south-west angle of the quadrant-room at the Royal Observatory at Greenwich: an angle being chosen, because the stone piers, to which the iron frame is screwed, have in such case but a small distance to project from the walls, in order to form an appui on both sides. The apparatus was employed in the two first experiments at Mr. Browne's house in London, where similar means could not be resorted to for rendering the point of suspension of the pendulum immoveable. In these experiments the agate planes were screwed to an iron plate, which was supported by four iron bars springing from the interior of the cylinder of the pedestal; and the bell glass rested on the upper glass cylinder, without the intervention of the suspension piece. It will be seen by the result of those two experiments, compared with the result of others in which

the point of suspension was immoveable, that the small motion of the support of the pendulum, occasioned by the elasticity of the iron bars, did not prejudice the comparative result of the vibration in air of different density: so that in all cases where a relative result only is required, the apparatus is effective, without the means resorted to at Greenwich to make the suspension immoveable.

Through the liberality of the Managers of the Royal Institution, the use of the air-pump belonging to the Institution was obtained for these experiments: and also an apparatus for the formation and supply of hydrogen gas, for purposes which will be described in their due succession. The experiments will be related in the order in which they were made, as being perhaps the most simple and perspicuous mode.

June 28, 1828.—On this day Mr. Newman having brought all the parts of the apparatus intended for the vacuum experiments to Portland Place, it was established in front of Mr. Browne's clock by Molyneux. The invariable pendulum No. 12. was placed on the agate planes numbered also 12, which were screwed fast to the iron plate supported by the four iron bars, and were carefully levelled. The thermometer, graduated by Mr. Daniell and myself, used in my former pendulum experiments, was suspended withinside the glasses, so that the ball was midway between the axis and the lower part of the weight of the pendulum; a mercurial gauge, commencing to act when the pressure was reduced to 10 inches, was also suspended. The pendulum was 1 foot 6 inches in front of the pendulum of the clock; and the telescope for observing coincidences was stationed in an adjoining room, 18 feet 6 inches from the detached pendulum, and 20 feet from the pendulum of the clock. A detached diaphragm, having a vertical opening, the sides of which viewed from the telescope were tangents to the disk, was placed between the vacuum apparatus and the disk; so that when the glasses were on, the disk and diaphragm were both seen from the telescope through the back and front of the lower glass cylinder. A graduated arc was placed with the diaphragm: the distance from the axis to the part of the pendulum crossed by the arc as seen from the telescope was 49.5 inches: the arc was divided into degrees, and the degrees were subdivided into spaces of 10′ each: the length of a degree was 0.73 inch: consequently the arc read off required to be multiplied by .845 to produce

the true arc of vibration. The telescope was then adjusted for the observation of coincidences, the glasses of the apparatus being on, and the joints pomatumed.

A preliminary trial was then made of the facility with which the air could be withdrawn. A double pump kept in steady action for fifteen minutes reduced the pressure to 7 inches. More was not then attempted; but on stopping the action of the pump, it was soon observed that a leak must exist, as the gauge rose at the rate of about an inch in seven or eight minutes. On intercepting the communication between the pump and the apparatus, the leak was shown to be in the latter. The air was then re-admitted; the joints examined, as well as the stuffing-box through which the wire passed which was employed to set the pendulum in motion. Mr. NEWMAN expressed himself satisfied that the leak could be only in the metal of the iron cylinder, notwithstanding the thickness of the metal was two inches. The further examination of the leak was postponed; and the pendulum prepared for the next day, when it was proposed to try its comparative vibration in air, and in a medium as rarefied as could be maintained in the then imperfect state of the apparatus.

EXP. I.—June 29. MOLYNEUX losing 0ˢ.17 per diem.—The glasses being on and prepared for exhausting, but a free communication existing with the external air through the exhausting pipe, the following coincidences were observed.

No. of Coincid.	Barom.	Therm.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and True Arc.	Mean Interval.	Correction for Arc.	Reduction to 72°.	Corrected Vibrations at 72°.
	inch.	°	m s	m s	h m s	° ′	s	s	s	
1	.....	.....	23 33	23 38	}	}				
2	29.91	70.6	29 42	29 48	} 9 29 45.17	2 06 = 1 47				
3	.....	.....	35 52	35 58	}	}	371.8	+3.06	−0.48	85937.62
15	.....	71.1	50 12	50 23	10 50 17.5	1 10 = 0 59.6				
31	29.90	71.3	29 40	30 00	12 29 50.0	0 35 = 0 29.7	373.28	+0.87	−0.34	85937.45
	29.905; Index + 0.066; Redᵃ to 32° − 0.111; = 29.861.								Vibrations at 72° =	85937.54

The barometer used in these and the subsequent experiments in London belonged to Mr. BROWNE. By several comparisons with the standard baro-

meters of the Royal Society and of the Royal Observatory, made by means of an intermediate portable barometer, an index correction of +0.066 was found to be required, to make Mr. BROWNE's barometer agree with the standards when corrected for capillary action. This correction is accordingly applied. In these and in all the subsequent experiments, both in London and at Greenwich, the registered arcs were obtained in the following manner:—The points of the graduated arc were noted, opposite to which the same side of the tail-piece of the pendulum stopped when at each extremity of its vibration; this gave the whole arc passed through by the pendulum without reference to a zero point: the half of this is the semi-arc of vibration. The same process was then gone through with the other side of the tail-piece of the pendulum; and a mean of the two semi-arcs is the arc registered. The true arc is the registered arc multiplied by .845.

The preliminary experiment in air being concluded, the pendulum was stopped by the wire passing through the stuffing-box, and again set in motion by the same at a true arc exceeding 1° 47′, being the arc with which the preceding experiment in air had commenced. The air was then withdrawn until the pressure was reduced to 7 inches. The thermometer, which had stood at 71°.4 before the process of withdrawing the air commenced, was observed to fall gradually, until it was reduced to 70°.7, when the pressure was 7 inches. The interval between successive coincidences being about six minutes, and the leak admitting sufficient air to cause the gauge to rise an inch in six minutes, whilst by working the pump gently, two inches could with ease be gained in the same time, the pump was worked, during the first 12 coincidences, only in the alternate intervals. From the 12th to the 39th coincidence, the gauge was kept always as near 7 inches as could be appreciated, by a very gentle and continued exercise of the pump.

No. of Coincid.	Therm.	Gauge.	Disapp.	Re-app.	Coincidence.	Interv.	Arc registered and True Arc.	Mean Interval.	Correction for Arc.	Temp. corrected.	Reduction to 75°.	Corrected vibrations at 75°.
	°	inch.	m s	m s	h m s		° °	s	s	s	s	
1	70.8	7.00	7 54	8 00	1 07 57	377	2 00 — 1 42					
2	.....	7.56	14 10	14 18	1 14 14	376.5						
3	.....	7.10	20 27	20 34	1 20 30.5	376.5						
4	.....	8.00	26 44	26 50	1 26 47	376	1 50					
5	71.1	7.00	33 00	33 06	1 33 03	377						
6	.....	8.10	39 16	39 24	1 39 20	377						
7	.....	7.00	45 33	45 41	1 45 37	377	1 43					
8	.....	8.00	51 50	51 58	1 51 54	376						
9	.....	6.70	58 06	58 14	1 58 10	378		377.29	+3.03	72.06	+0.02	85944.85
10	.....	7.90	4 24	4 32	2 04 28	377						
11	71.5	5.60	10 40	10 50	2 10 45	378						
12	.....	7.00	16 58	17 08	2 17 03	378						
13	.....	7.00	23 16	23 26	2 23 21	377.5	1 28					
14	.....	7.00	29 34	29 43	2 29 38.5	377.5						
15	.....	7.00	35 51	36 01	2 35 56	377.5						
16	71.6	7.00	42 08	42 19	2 42 13.5	377.5						
17	.....	7.10	48 26	48 36	2 48 31	379						
18	.....	7.10	54 44	54 56	2 54 50	377.5						
19	71.8	7.10				378	1 14 — 1 03					
20	.....	7.00	7 20	7 31	3 07 25.5	378						
21	.....	6.90				378.5						
22	.....	6.90	19 57	20 07	3 20 02	378						
23	.....	6.90				378.5						
24	72.0	6.90	32 33	32 44	3 32 38.5	378.5						
25	.....	6.90				378.5						
26	.....	7.10	45 10	45 21	3 45 15.5	378.5	1 03					
27	.....	7.50				379						
28	.....	6.90	57 47	57 59	3 57 53	378.5						
29	.....	7.00				378.5						
30	72.3	7.00	10 24	10 36	4 10 30	379		378.55	+1.11	72.94	+0.40	85944.85
31	.....	7.00	16 42	16 56	4 16 49	378						
32	.....	6.90				378.5						
33	.....	7.00	29 18	29 33	4 29 25.5	379	0 52					
34	.....	7.00	35 37	35 52	4 35 44.5	379						
35	.....	6.90	41 56	42 11	4 42 03.5	378.5						
36	.....	7.00				379						
37	72.5	7.00	54 33	54 49	4 54 41	378.5						
38	.....	7.00				378.5	0 44 — 0 37					
39	72.6	7.00	7 11	7 25	5 07 18							85944.85
		7.08										

On re-admitting the air, the thermometer rose from 72°.6 to 73°.8, and then gradually fell until it took up the temperature of the apartment, which was rather less than 73°.5. After the lapse of half an hour, the following observations were commenced, the glasses remaining on, but a free communication existing with the external air through the exhausting pipe.

No. of Coincid.	Therm.	Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and True Arc.	Mean Interval.	Correction for Arc.	Reduction to 75°.	Corrected Vibrations at 75°.
	°	inch.	m s	m s	h m s	° ′ ″ ° ′	s	s	s	
1	73.6	29.90	38 14	38 20	5 38 17	2 02 = 1 44	370.77	+3.06	+0.55	85937.37
12	73.0		46 10	46 21	6 46 15.5	1 15 = 1 04	372.57	+1.11	+0.31	85937.44
26	72.5	29.86	13 00	13 23	8 13 11.5	0 43 = 0 36				
	73.03	29.88; Index + 0.066; Reduction to 32° − 0.117; = 29.829.								85937.40

The glasses were then removed, and the pendulum raised on the Y's; in which operation it was observed that it had not quitted its place during the experiments, in which it had been twice set in motion and twice stopped by the wire which passes through the stuffing-box. The pendulum was then removed, and the horizontality of the planes examined and found perfectly correct.

The vibrations in air, before and after those in the rarefied medium, were as follows:

In the morning, before the vibration in the rarefied medium	85937.54;	Barom. 29.861
In the evening, after the vibration in the rarefied medium	85937.40;	Barom. 29.829
Mean . . .	85937.47;	Barom. 29.845
The vibrations in the rarefied medium, reduced to the same temp. as those in air . .	85944.85;	Gauge 7.08

Whence there appears, as the result of this experiment, a difference of 7.38 vibrations per diem, corresponding to a difference of atmospheric pressure of 22.765 inches: the temperature of the air of full pressure being 72°.91; and that of the rarefied air 72°.5.

The indications of the thermometer in the rarefied medium have been increased 0°.7, to compensate the effect produced on the thermometer by the removal of the full pressure of the atmosphere. It has been noticed, that on the pump being worked, the thermometer, which previously stood at 71°.4, fell to 70°.7, which it indicated when the pressure was reduced to 7 inches. The converse took place when the air was re-admitted. To ascertain whether this effect was rightly ascribed to the removal of the pressure of the atmosphere on the exterior of the ball and tube of the thermometer, the following experiment

was made:—The thermometer being immersed in pounded ice, and placed on the brass plate of an air-pump, the mercury coincided exactly with the division of 32°: it was then covered with a receiver, and the air withdrawn: the thermometer fell as the pump was worked; and when the gauge indicated a pressure of half an inch, the mercury stood at 31°.25: on re-admitting the air it rose again to 32°. The experiment was repeated with precisely similar results. By observing carefully the indications of the thermometer with those of the gauge, the following corrections of the thermometer were assigned for different pressures: for a near approach to a vacuum + 0°.75; for 7 inches and thereabouts + 0°.70; for 15 inches and thereabouts + 0.5; and for 20 inches + 0°.4. The propriety of these corrections was subsequently confirmed, in the experiments with the vacuum apparatus at Greenwich which will be related in the sequel, by registering always the comparative indications of the thermometer which had been tried in ice, and of two others included in a glass cylinder, which had been hermetically closed under the receiver of an air-pump when the air was withdrawn. The cylinder including these thermometers being suspended by the side of the standard in the vacuum apparatus, the doubly inclosed thermometers underwent no change on the exhaustion of the apparatus; whilst the standard thermometer fell an amount corresponding to the above corrections, and remained permanently lower than the others to the same amount, until the air was re-admitted, when the indications of the three agreed*.

The result of the experiment on the 29th of June then was, a difference of 7.38 vibrations for a difference of pressure of atmospheric air at 72°, corresponding to 22.765 inches of mercury at 32°: this result is equivalent to the reduction to a vacuum, for the vibration in a pressure of 30 inches of air of 72°, of 9.725 vibrations per diem.

The specific gravity of the pendulum being about 8.6; and the weight of water to that of air, at 29.27 inches of the barometer, and 53° of the thermometer, as 836 to 1, and the expansion of air for each degree of the thermometer $\frac{1}{480}$dth of its bulk, the correction for the buoyancy of an atmosphere of 30 inches of air

* On trying a thermometer with a ball of unusually large diameter in the pounded ice, the removal of the pressure of the atmosphere made a difference in the height of the mercury at the freezing point, amounting fully to 1° of its scale.

at 72°, is 5.88 vibrations. The difference, or 3.845 vibrations per diem, is the amount by which the experimental reduction to a vacuum exceeds the reduction which it has been customary to compute.

From the imperfect state of the apparatus in this first experiment, doubts might have been entertained of the correctness of the result on two accounts: it might have been supposed, 1st, that the abstraction of the air being kept up continually, during the vibration in the rarefied medium, to counteract the leakage, currents might have been occasioned influencing the time of vibration: or, 2nd, the iron bars supporting the pendulum not having sufficient spread at the bottom to counteract the lateral force arising from the vibration, and the point of suspension itself partaking of it in consequence, it might have been supposed that the time of vibration was unequally affected thereby in the air and in the rarefied medium. By experiments made with the same pendulum on the 8th and 9th of July, an account of which is already before the Society, (Phil. Trans. 1829, Art. IX.) in which experiments the pendulum was suspended from Captain Kater's original mahogany support in the same room, the vibrations on an immoveable support, all other circumstances being the same, were found to exceed those on the plate of the vacuum apparatus, by about 18 vibrations a day; due, doubtless, to the motion of the plate during the vibration, arising from the elasticity of the iron bars and their insufficient spread. To give more firmness to the suspension in the vacuum apparatus in a second experiment, inch boards of well seasoned oak were inserted vertically, having their lower ends resting on the interior of the iron cylinder which supports the glasses, and the plate was screwed down firmly on their upper ends by screws working into the iron bars: the suspension plate was thus directly and firmly connected with the foot cylinder by means of the boards independently of the bars; the boards being hollowed out in the proper places to admit the observation of coincidences.

To detect where the leakage took place, the interior of the apparatus was filled with water as high as the lower glass cylinder, and a communication being established between the exhausting pipe and the upper part of the interior, the air was withdrawn; when bubbles of air were seen to rise rapidly from the interior surface of the iron foot cylinder, particularly from those parts of it which were opposite the flanches on the outside, where the metal was

thickest. An iron cement, composed of cast iron filings and white lead, was then rubbed strongly into the porous parts of the cast iron; and several coats of oil paint were given successively, both to the outside and inside of the cylinder. These alterations having been made, the apparatus was again established in front of the clock, and prepared for a second experiment.

Exp. II.—July 13th; Molyneux losing 0.2 per diem.

Every thing was arranged on this day as in the first experiment, with the exception of the above alterations, and the removal of the telescope for observing coincidences nearer the pendulum, from which it was now distant 11 feet. The diaphragm and arc were also placed within the glasses, instead of being between the glasses and the clock. The graduated arc was 48 inches below the point of suspension, whence the registered arc required to be multiplied by .87 to produce the true arc. The preliminary vibration in air was as follows.

No. of Coincid.	Therm.	Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 70°.	Corrected Vibrations at 70°.
	°	inch.	m s	m s	h m s	° ′ ° ′	s	s	s	
1	70.0	29.43	58 05	58 12	0 58 08.5	1 34.5 = 1 22	382.12	+1.40	−0.10	85950.86
22	69.5	29.46	11 43	12 03	3 11 53.0	0 370. = 0 32				
	69.75	29.445; Index + 0.066; Reduction to 32° − 0.107; = 29.404.								85950.86

The air was then withdrawn, and the following observations made.

No. of Coincid.	Gauge.	Therm.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 70°.	Corrected Vibrations at 70°.
	inch.	°	m s	m s	h m s	° ′ ° ′	s	s	s	
1	1.50	68.8	18 31	18 38	4 18 34.5	1 41 = 1 28	389.73	+1.73	−0.25	85959.88
19	1.70	69.0	15 13	15 23	6 15 18	1 16				
32	1.75	68.8	39 39	39 53	7 39 46	1 04.5				
59	1.80	68.0	35 19	35 39	10 35 29	0 44 = 0 38	391.52	+0.24	−0.71	85959.95
149	1.80	67.1	22 31	23 01	20 22 46	0 12 = 0 10				
	1.71	68.34 + 0.75 = 69.09								85959.913

The air was then admitted, and in the afternoon (July 14) the following observations made.

No. of Coincid.	Therm.	Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 70°.	Corrected Vibrations at 70°.
	°	inch.	m s	m s	h m s	° ′ = ° ′	s	s	s	
1	69.9	29.50	45 29	45 38	2 45 33.5	1 18 = 1 08	382.52	+0.95	−0.15	85950.84
22	69.4	29.50	59 15	59 38	4 59 26.5	0 33 = 0 26				
	69.65	29.50; Index + 0.066; Reduction to 32° − 0.107; = 29.459.								85950.84

The vibrations in air in this experiment were as follows:

July 13. Previous to the vibration in a rarefied medium . . . . . } 85950.86; Barom. 29.404 inches.

July 14. Subsequent to the vibration in a rarefied medium . . . . . } 85950.84; Barom. 29.459

Mean . . 85950.85; Barom. 29.431

The vibrations in a rarefied medium reduced to the same temperature as those in air } 85959.915; Gauge 1.71 inches.

Whence there appears, as the result of this experiment, a difference of 9.065 vibrations per diem, corresponding to a difference of atmospheric pressure of 27.721 inches: the temperature of the air of full pressure being 69°.7, and that of the rarefied air 69°.09. This result is equivalent to the reduction to a vacuum for the vibration in 30 inches of air of that temperature, of 9.81 vibrations per diem. The "correction for buoyancy" is 5.92 vibrations.

The cement and paint had been effectual in preventing the leakage; from half past ten on the evening of the 13th to half past eight on the following morning, as shown in the table, and subsequently until 1 P.M. on the same day, when the pendulum was still vibrating, but in an arc too small to admit of the observation of coincidences, the gauge at 1.8 underwent no perceptible change. The introduction of the oak boards had also contributed considerably to the firmness of the suspension plate: the excess of vibration on an immoveable support being reduced from 18 vibrations a day in the former experiment to 5½ in the present; whilst the accordance of the results on the two occasions furnishing the reduction to a vacuum, gave reason to conclude, that the comparative

vibration in the air of full pressure and in rarefied air was not sensibly affected by a small motion of the support.

It was now considered, therefore, as established by the experiments, that the true reduction to a vacuum is considerably greater than it had been customary to suppose; for the invariable pendulum, for example, nearly as 5 to 3. It was also obvious, that all pendulums whatsoever, employed in air, and designed to give results which should be independent of the variable retardation occasioned by their vibration in air, would require to have the influence of the air on their respective vibrations, ascertained by experiment, since it is not attainable by calculation. Now as the apparatus was suited by its construction, to furnish this element with facility and accuracy, for any of the forms in which pendulums have hitherto been made, either for determining absolute or relative lengths, it was probable that it might eventually become more extensively useful, than in its present office of furnishing the reduction for an invariable pendulum.

It was thought proper, therefore, that the apparatus should be removed to the Royal Observatory at Greenwich and established there, in order that it might be hereafter at the command of persons to whom it might be useful, upon their application to the Board of Longitude at whose expense it had been constructed. The iron suspension plate with the iron bars supporting it were now removed, and the bell metal plate with the circular exterior ring, represented in Plate VI, substituted, with the iron frame-work and screws, as represented elsewhere in the plate, enabling the support of the pendulum to be fixed immoveably at pleasure in the manner already described. A clock by Dent, with a mercurial pendulum carrying a disk, was placed in the angle behind the apparatus; and the telescope for observing coincidences in front, about 16 feet distant from the detached pendulum when suspended. Arrangements were made for observing the coincidences by artificial light, without interfering with the temperature of the room, by directing the light of an Argand lamp, stationed in an adjoining apartment, on the disk of the clock pendulum, through a tin tube, which prevented the diffusion of the light in the room. The diaphragm was placed between the glasses and the clock, and the arc within the glasses close to the pendulum. The arc was graduated in inches and tenths, and was read off to hundredths; crossing the pendulum at 47.7 inches from

the point of suspension, the registered divisions multiplied by 1.2 give the arc in degrees and parts. In addition to the mercurial gauge, a mercurial barometer was suspended within the glasses, having a glass tube and cistern, the latter sufficiently capacious to receive, if necessary, the whole of the mercury in the tube; an inch of mercury descending from the tube raised the level of the mercury in the cistern $\frac{1}{11}$th of an inch: the scale was marked in red lines on the glass tube. The range of the mercurial gauge not exceeding 10 inches, the barometer was necessary for pressures between 10 inches and the full pressure of the external atmosphere. Comparing it, when suspended in its place, with the standard barometer of the Observatory, its indication, at about 30 inches, was found to require an additive correction of 0.32 inch; the standard being corrected for capillary action, but the barometer of the apparatus uncorrected, as the interior diameter of the tube was not precisely known. The air being then withdrawn from the apparatus until the gauge was brought in action, the barometer was found to require an additive correction of 0.41 inch after the correction for the level of the cistern, to make it agree with the mean indication of the two legs of the gauge; which mean was observed throughout. This barometer being only used at 14 inches and thereabouts, an additive correction of $\frac{0.32+0.41}{2} = 0.36$ is applied to its registry; which may be presumed to give a comparative indication to the gauge and standard barometer within a tenth of an inch. The two thermometers inclosed in a sealed glass cylinder, from the interior of which the air had been withdrawn, were suspended by the side of the standard thermometer: these thermometers are numbered 2 and 3 in the subsequent tables; the standard is No. 1; and an exterior thermometer, suspended in the free air near the apparatus, and at the same level as the thermometers within the glasses, is No. 4.

In consequence of my absence from England, the experiments with the invariable pendulum in the apparatus were suspended until January of the present year, when they were resumed with the valuable assistance and cooperation of Mr. Thomas Glanville Taylor of the Royal Observatory, whose observations are distinguished in the subsequent pages by his name. The invariable pendulum No. 12, employed in the preceding experiments, being at this time engaged in other determinations, I obtained permission to detain and employ

for the present purpose, a similar pendulum, No. 13, destined eventually for the Brussels Observatory.

The arrangement of the observations in each of the succeeding experiments was the same as in those already related; the pendulum was first vibrated in air of full pressure; then in a rarefied medium; and lastly, again, in the air. A mean was then taken between the two series in air; with which mean, the intermediate vibration in the rarefied medium was compared. The result of the comparison was thus wholly independent of the daily rate of the clock; and in some measure also, of its deviations from an uniform rate in intervals less than 24 hours. The clock was compared daily with the transit clock of the observatory; but as the weather in the last half of January and first half of February was very rarely clear, and as the transit clock about that period was more than usually irregular in its going, it has been deemed preferable to take a mean rate for the coincidence clock for the months of January and February; its deviations from this mean rate on the days of experiment are transferred in appearance to the going of the pendulum on different days: this apparent irregularity is however wholly inconsequential in respect to the purpose of the present experiments, for which it is only necessary that the vibrations of the pendulum should be relative in the three series of coincidences forming each distinct experiment.

EXP. III.—Greenwich, January 14th and 15th. Clock gaining $4^m\ 14^s.38$.

Observer.	No. of Coin.	Thermometers. 1	2	3	4	Standard Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduc. to 36°.	Corrected Vibrations at 36°.
		°			°	inch.	m s	m s		°				
Captain SABINE.	1	.....	.....	.....	.....	.....	00 52	01 07	h m s	Div. °				
	2	35.4	.....	.....	35.7	29.93	09 05	09 19	11 09 12.83	0.60 = 0.83				
	3	.....	.....	.....	.....	.....	17 21	17 33						
	12	35.6	.....	.....	35.6	.....	.....	.....			494.76	+0.74	−0.20	86304.60
	20	.....	.....	.....	.....	.....	37 22	37 51						
	21	.....	.....	.....	.....	.....	45 39	46 07						
	22	35.6	.....	.....	35.7	29.90	53 52	54 22	1 54 07.9	0.45 = 0.53				
	23	.....	.....	.....	.....	.....	02 11	02 37						
	24	.....	.....	.....	.....	.....	10 24	10 54						
		35.53				29.915; Capill. + 0.019; Reduction to 32° − 0.017; = 29.917.								86304.60

The air was then withdrawn, the pendulum again set in motion, and suffered to vibrate an hour before the registry was commenced.

Observers.	No. of Coincid.	Thermometers. 1	2	3	4	Gauge.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 32°.	Corrected Vibrations in 32°.
		°	°	°	°	inch.	m s	m s	h m s	Div. °				
Captain Sabine.	1	35.5	36.4	36.6	36.7	1.56	30 14	30 26						
	2	.....	.....	.....	.....	.....	38 40	38 53	3 47 13.62	0.87 = 1.04				
	3	.....	.....	.....	.....	.....	.....	.....						
	4	.....	.....	.....	.....	.....	55 33	55 48						
	5	36.0	36.7	36.9	36.7	1.69	04 00	04 16			507.3	+1.33	+0.23	86314.30
														Gauge 1.88
Captain Sabine.	17	35.8	36.4	36.7	36.3	2.10	45 27	45 53						
	18	.....	.....	.....	.....	.....	53 53	54 14						
	19	.....	.....	.....	.....	.....	.....	.....	6 02 30.43	0.63 = 0.76				
	20	.....	.....	.....	.....	.....	10 48	11 07						
	21	35.9	36.5	36.8	36.6	2.17	19 15	19 36			507.6	+0.64	+0.26	86313.84
	36	35.8	36.4	36.6	36.5	2.62	26 06	26 32						Gauge 2.395
Captain Sabine.	37	.....	.....	.....	.....	.....	34 34	35 00						
	38	.....	.....	.....	.....	.....	43 01	43 28	8 43 14.73	0.42 = 0.50				
	39	.....	.....	.....	.....	.....	51 28	51 57						
	40	36.0	36.6	36.9	36.8	2.69	59 57	00 23			508.0	+0.25	+0.19	86313.65
Mr. Taylor.	60	.....	.....	.....	.....	3.10	49 16	49 46						Gauge 2.88
	61	35.5	36.2	36.3	36.2	.....	57 45	58 13	11 57 58.67	0.24 = 0.29				
	62	.....	.....	.....	.....	3.12	06 13	06 39						
		35.75 + 0.75 = 36.50				2.38								86313.93

The air was then admitted, and the following observations were made on the next morning.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Stand. and Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 32°.	Corrected Vibrations in 32°.
		°	°	°	°	inch.	m s	m s	h m s	Div. °				
Mr. Taylor.	1	.....	.....	.....	.....	.....	06 58	07 12						
	2	36.1	36.2	36.2	36.4	29.76	15 12	15 26	0 15 19	0.675=0.81				
	3	.....	.....	.....	.....	.....	23 26	23 40			494.22	+0.36	+0.10	86304.18
Mr. Taylor.	27	.....	.....	.....	.....	.....	40 51	41 38						
	28	36.4	.....	.....	36.5	29.76	49 05	49 54	3 49 28.83	0.17 = 0.20				
	29	.....	.....	.....	.....	.....	57 18	58 07						
		36.25				29.76; Capill. + 0.019; Reduction to 32° − 0.018; = 29.761.								86304.18

The vibrations in air in this experiment were as follows:

January 14. Previous to the vibration in the rarefied medium	86304.60;	Barom. 29.917 inches.
January 15. Subsequent to the vibration in the rarefied medium	86304.18;	Barom. 29.761
Mean . .	86304.39;	Barom. 29.839

The vibrations in the rarefied medium, reduced to the same temperature as those in air	86313.93;	Gauge 2.38 inches.

Whence it appears as the result of this experiment, that a difference of 9.54 vibrations per diem corresponds to a difference of atmospheric pressure of 27.459 inches of mercury at 32°; the temperature of the air of full pressure being 35°.89, and that of the rarefied air 36°.5.

The trifling leakage of the apparatus in this and the following experiment was attributed, and as it was afterwards proved justly, to the severity of the weather, which hardened the pomatum so that it did not secure the joinings of the glasses so well as usual.

Exp. IV.—January 17th and 18th. Clock gaining $4^m\ 14^s.38$.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Standard Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	inch.	m s	m s		Div.				
Captain Sabine.	1	.....	.....	.....	.....	.....	16 24	16 36						
	2	32.3	32.2	32.4	32.4	29.65	24 38	24 53	h m s 11 24 44.67	0.785 = 0.94	s 496.08	+0.64	−1.54	86304.13
	3	.....	.....	.....	.....	.....	32 51	33 06						
	19	.....	.....	.....	.....	.....	45 04	45 28						
	20	32.4	32.3	32.5	32.4	29.66	53 21	53 49	1 53 34	0.29 = 0.35				
	21	.....	.....	.....	.....	.....	01 37	02 05						
		32.35				29.655; Capill. + 0.019; Reduction to 32° − 0.004; = 29.670.								86304.13

The air was then withdrawn, and the following observations made; the valves of the pump requiring repair, the gauge could not be reduced lower than 3 inches.

Observers.	No. of Coincid.	Thermometers. 1	2	3	4	Gauge.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	inch.	m s	m s		Div.				
Captain Sabine.	1	.....	.....	.....	.....	.....	07 35	07 48						
	2	31.7	32.3	32.5	32.6	3.09	16 03	16 18	h m s 3 16 10.17	0.85 = 1.02				
	3	.....	.....	.....	.....	.....	24 31	24 46			s 508.55	+1.14	−1.47	86313.25
Captain Sabine.	20	.....	.....	.....	.....	.....	48 33	48 55						Gauge 3.29
	21	31.8	32.2	32.4	32.4	3.48	57 01	57 25	5 57 12.83	0.55 = 0.66				
	22	.....	.....	.....	.....	.....	05 29	05 54						
Mr. Taylor.	60	.....	.....	.....	.....	.....	28 16	28 36						
	61	31.6	32.2	32.2	32.4	4.19	36 48	37 08	11 36 58.33	0.23 = 0.28	s 509.64	+0.34	−1.49	86313.16
	62	.....	.....	.....	.....	.....	45 20	45 42						Gauge 3.84
		31.7; + 0.75 = 32.45				3.59								86313.20

The air was then admitted, and the following observations made the next morning.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Stand. and Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 36°.	Corrected Vibrations at 36°.
		°	°	°	°	inch.	m s	m s						
Mr. Taylor.	1	.....	.....	.....	.....	.....	6 27	6 37		Div.	s			
	2	32.8	33.0	33.0	31.8	29.69	14 43	14 51	h m s 23 14 47.33	0.76 = 0.91	495.92	+0.55	−1.52	86303.94
	3	.....	.....	.....	.....	.....	22 59	23 07						
	23	.....	.....	.....	.....	.....	8 14	8 27						
	24	32.0	32.1	32.2	32.3	29.85	16 31	16 44	2 16 37.5	0.25 = 0.30				
	25	.....	.....	.....	.....	.....	24 48	25 01						
		32.4				29.77; Capill. + 0.019; Reduction to 32° − 0.002; = 29.787.								86303.94

The vibrations in air in this experiment were as follows:

January 17. Previous to the vibration in the rarefied medium . . . . . } 86304.13; Barom. 29.670 inches.

January 18. Subsequent to the vibration in the rarefied medium . . . . . } 86303.94; Barom. 29.787

86304.03; Barom. 29.728

The vibrations in a rarefied medium, reduced to the same temp. as those in air } 86313.20; Gauge 3.59 inches.

Whence the result, that a difference of 9.17 vibrations per diem corresponds to a difference of atmospheric pressure of 26.138 inches of mercury at 32°: the temperature of the air of full pressure being 32°.37, and that of the rarefied air 32°.45.

Exp. V.—January 30th and 31st. Clock gaining $4^m\ 14^s.38$. In air.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Stand. and Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 36°.	Corrected Vibrations at 36°.
		°	°	°	°	inch.	m s	m s						
Captain Sabine.	1	.....	.....	.....	.....	.....	55 20	55 32		Div.	s			
	2	36.0	.....	.....	36.1	29.44	3 35	3 47	h m s 11 03 40.67	0.88 = 1.06	496.35	+0.68	+0.08	86305.97
	3	.....	.....	.....	.....	.....	11 49	12 01						
	25	.....	.....	.....	.....	.....	13 43	14 08						
	26	36.4	.....	.....	36.4	29.50	21 59	22 28	2 22 13.17	0.25 = 0.30				
	27	.....	.....	.....	.....	.....	30 14	30 47						
		36.2				29.47; Capill. + 0.019; Reduction to 32° − 0.015; = 29.474.								86305.97

The air was then withdrawn, and the following observations made. The air-pump had been repaired, but 0.6 inch was the limit to which exhaustion could be carried by its means. The leakage of the apparatus did not exceed seven hundredths of an inch in the 18 hours of the experiments.

Observers.	No. of Coincid.	Thermometers. 1	2	3	4	Gauge.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	inch.	m s	m s	h m s	Div.				
Captain Sabine.	1	.....				.....	14 19	14 31						
	2	35.9	36.6	36.7	37.4	0.63	22 45	23 01	3 22 53.33	0.91 = 1.09				
	3	.....				.....	31 16	31 28			510.42	+1.00	+0.27	86316.11
Mr. Taylor.	62	.....				.....	53 02	53 34						Gauge 0.63
	63	35.9	36.4	36.5	37.3	0.63	01 33	02 05	12 01 49.17	0.415 = 0.50				
	64	.....				.....	10 05	10 36			512.08	+0.18	+0.19	86316.31
Mr. Taylor.	127	.....				.....	7 42	8 25						Gauge 0.665
	128	35.5	36.4	36.5	36.6	0.70	16 21	16 47	21 16 34.17	0.17 = 0.20				
	129	.....				.....	24 51	25 19						
		35.77; + 0.75 = 36.52				0.653								86316.21

The air was then re-admitted, and the following observations made.

Observers.	No. of Coincid.	Thermometers. 1	2	3	4	Stand. and Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	inch.	m s	m s	h m s	Div.				
Mr. Taylor.	1	.....				.....	53 33	53 43						
	2	36.7	36.6	36.7	36.7	30.03	01 45	01 55	22 01 50.83	0.80 = 0.96				
	3	.....				.....	09 59	10 10			496.0	+0.49	+0.25	86305.69
	26	.....				.....	19 58	20 26						
Captain Sabine.	27	.....				.....	28 16	28 43						
	28	36.5	36.5	36.6	36.5	30.07	.....	.....	1 36 46.5	0.185 = 0.22				
	29	.....				.....	44 50	45 19						
	30	.....				.....	53 03	53 37						
		36.6				30.05; Capill. + 0.019; Reduction to 32° − 0.018; = 30.051.								86305.69

The vibrations in this experiment were as follows:

January 30. Previous to the vibration in the rarefied medium . . . . . } 86305.97; Barom. 29.474 inches.

January 31. Subsequent to the vibration in the rarefied medium . . . . . } 86305.69; Barom. 30.051

Mean . . . 86305.83; Barom. 29.762

The vibrations in a rarefied medium reduced to the same temperature as those in air } 86316.21; Gauge 0.653 inches.

Whence there appears as the result of this experiment, a difference of 10.38 vibrations per diem, corresponding to a difference of atmospheric pressure of 29.109 inches of mercury at 32°; the temperature of the air of full pressure being 36°.4; and that of the rarefied air, 36°.52.

Exp. VI.—January 31st (continued). Clock gaining $4^m\ 14^s.38$.

As soon as the concluding observation in air of the preceding experiment was completed, the air was withdrawn until the included barometer indicated a pressure of about half an atmosphere; when the following observations were made.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Included Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	Inch.	m s	m s						
Captain Sabine.	1	.....	.....	.....	.....	.....	56 11	56 21	h m s	Div.				
	2	37.0	37.6	37.8	37.0	13.90	04 30	04 42	3 04 36.00	0.91 = 1.09				
	3	.....	.....	.....	.....	.....	12 49	13 03						
		36.0	36.5	36.8	35.7	.....					s	s	s	
Mr. Taylor.	62	.....	.....	.....	.....	.....	28 32	29 02			504.186	+0.43	+0.26	86311.35
	63	35.4	36.0	36.0	34.6	13.91	26 57	27 27	11 37 11.33	0.095 = 0.114				
	64	.....	.....	.....	.....	.....	45 22	45 48						
		36.13; + 0.5 = 36.63				13.905; Cistern − 0.144; Index + 0.360; Red^n to 32° − 0.005; = 14.116.								86311.35

The air was then admitted; and on the next day, February 1, the following observations were made.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Standard Barom.	Times of Disapp.	Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	Inch.	m s	m s						
Mr. Taylor.	1	.....	.....	.....	.....	.....	37 18	37 27	h m s	Div.				
	2	34.7	.....	.....	35.2	30.32	45 32	45 41	22 45 36.83	0.89 = 1.07				
	3	.....	.....	.....	.....	.....	53 47	53 56						
	23	.....	.....	.....	.....	.....	39 17	39 26			s	s	s	
	24	34.8	.....	.....	34.8	30.35	47 35	47 51	1 47 43.5	0.255 = 0.30	496.67	+0.69	−0.53	86305.60
	25	.....	.....	.....	.....	.....	55 52	56 10						
		34.75				30.335; Capill. + 0.019; Reduction to 32° − 0.012; = 30.342.								86305.60

The vibrations in this experiment were as follows:

January 31. Previous to the vibration in the rarefied medium	86305.69;	Barom. 30.051 inches.
February 1. Subsequent to the vibration in the rarefied medium	86305.60;	Barom. 30.342
Mean . . .	86305.645;	Barom. 30.196
The vibrations in a rarefied medium reduced to the same temperature as those in air	86311.35;	Barom. 14.116 inches.

Whence there appears as the result of this experiment, a difference of 5.705 vibrations per diem, corresponding to a difference of atmospheric pressure of 16.08 inches of mercury at 32°; the temperature of the air of full pressure being 35°.67, and that of the rarefied medium 36°.63.

Exp. VII.—February 9th and 10th. Clock gaining $4^m\ 14^s.38$.

This experiment was undertaken for a distinct purpose; that of ascertaining the comparative retardation of a pendulum vibrating in an atmosphere of hydrogen gas, and in an atmosphere of common air. It had been suggested to me by Mr. William Hasledyne Pepys, that a nearer practical approach to the vibration in a vacuum, than the pump had hitherto effected, might be accomplished, by filling the apparatus with hydrogen gas, and pumping out to the extent that the pump could carry the process of exhaustion; when the small portion of the gas remaining in the apparatus, being 13 times lighter than a remainder of air which would effect the gauge to an equal amount, might be expected to have an influence on the vibration diminished in the ratio of the respective specific gravities of air and hydrogen. To ascertain, therefore, whether the retardations of air and hydrogen gas were in that ratio was the object of this experiment, which was accomplished by vibrating the pendulum, 1st, in the ordinary atmosphere; and 2nd, in an atmosphere of hydrogen gas, of equal pressure, or as nearly so as circumstances would permit.

First, in air.

Observer.	No. of Coincid.	Thermometers. 1	Thermometers. 4	Standard Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to the Zenith.	Corrected Vibrations at 30°.
		°	°	inch.	m s	m s						
Captain Sabine.	1	.....	.....	.....	14 45	14 58		Div.				
	2	37.7	37.8	30.200	22 58	23 12	h m s 21 23 05.17	0.85 = 1.02				
	3	.....	.....	.....	31 13	31 25			494.745	+0.61	+0.88	86305.57
	22	.....	.....	.....	7 43	8 16						
	23	38.5	38.5	30.156	15 58	16 32	0 16 14.83	0.235 = 0.28				
	24	.....	.....	.....	24 14	24 46						
		38.1		30.193; Capill. + 0.019; Reduction to 32° − 0.019; = 30.193.								86305.57

The air was then withdrawn until the gauge indicated 0.7 inch; and hydrogen gas was introduced, passed through a cylinder containing muriate of lime, till the mercury in the barometer tube rose to 30 inches. The gas was

then pumped out till the gauge indicated 1.4 inch. A fresh supply of gas was then introduced, passing through the cylinder containing fresh muriate of lime, till the pressure withinside the apparatus exceeded by about 0.2 inch the pressure of the atmosphere on the exterior. An equilibrium of pressure was then produced, by permitting the escape of the small portion of gas necessary for that purpose. The mercury in the included barometer stood at 29.95, corresponding to the indication of the standard barometer of 30.24 inches at 32°. The following observations were then made.

Observers.	No. of Coincid.	Thermometers. 1	Thermometers. 4	Included Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	inch.	m s	m s	h m s	Div. °	s	s	s	
Captain Sabine.	1	.....	.....	.....	22 29	22 39						
	2	40.0	40.0	29.95	30 50	31 03	8 30 56.5	0.96 = 1.15				
	3	.....	.....	.....	39 13	39 25			505.03	+1.11	+1.60	86313.93
Mr. Taylor.	35	.....	.....	.....	8 35	8 50						
	36	39.6	39.7	29.90	17 03	17 15	13 17 07.67	0.44 = 0.53				
	37	.....	.....	.....	25 25	25 38						
		39.8		29.925; Index + 0.32; Reduction to 32° − 0.03; = 30.215.								86313.93

The apparatus was then left during the night filled with the gas, and on the next morning a fresh impulse was given to the pendulum, and the observation repeated.

Observers.	No. of Coincid.	Thermometers. 1	Thermometers. 4	Included Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduction to 30°.	Corrected Vibrations at 30°.
		°	°	inch.	m s	m s	h m s	Div. °	s	s	s	
Mr. Taylor.	1	.....	.....	.....	54 46	54 58						
	2	38.8	38.8	29.88	03 12	03 24	0 03 18	0.86 = 1.03				
	3	.....	.....	.....	11 38	11 50						
	35	.....	.....	.....	41 43	42 07			506.64	+0.73	+1.20	86314.24
	36	.....	.....	.....	50 09	50 37						
Captain Sabine.	37	38.9	39.0	29.87	58 34	59 07	4 58 50.5	0.30 = 0.36				
	38	.....	.....	.....	07 00	07 36						
	39	.....	.....	.....	15 26	16 06						
		38.85		29.875; Index + 0.32; Reduction to 32° −0.025; = 30.17.								86314.24

The vibrations in this experiment were as follows:

In hydrogen gas { February 9 . . 86313.93; Barom. 30.215 inches.
February 10 . . 86314.24; Barom. 30.170

Mean . . 86314.085; Barom. 30.1925

In atmospheric air, February 9 . . 86305.57; Barom. 30.193 inches.

Whence it appears that at the same height of the barometer, 30.193 inches, the pendulum made 8.515 vibrations per diem more in hydrogen gas than in atmospheric air; the temperature of the gas being 39°.32, and of the air 38°.1.

The hydrogen gas was obtained under Mr. NEWMAN's superintendence, by the action of zinc upon dilute sulphuric acid; and was collected in a gasometer previous to its transfer into the apparatus, which was effected through the metallic pipe usually connected with the air-pump. The cylinder containing muriate of lime was made a part of the communication between the apparatus and the gasometer.

EXP. VIII.—February 10th, 11th, and 12th. Clock gaining $4^m\ 14^s.38$.

The hydrogen having been again pumped out till the gauge showed 0.7 inch, a fresh atmosphere of hydrogen was introduced by Mr. NEWMAN with every possible care to ensure its purity; when the following observations were made.

Observer.	No. of Coll.	Thermometer. 1	Thermometer. 4	Included Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 30°.	Corrected Vibrations at 30°.
		°	°	inch.	m s	m s	h m s					
Mr. TAYLOR.	1	.....	.....	.....	15 56	16 15		Div.				
	2	39.8	39.7	29.84	24 23	24 39	11 24 31.33	0.765 = 0.92				
	3	.....	.....	.....	32 48	33 05						
	80	.....	.....	.....	23 25	25 17						
	81	.....	.....	.....	32 04	33 54						
	82	.....	.....	.....	40 42	42 21						
	83	39.7	39.7	29.82	49 07	50 40	22 49 55.43	0.107 = 0.128				
	84	.....	.....	.....	57 44	59 04						
	85	.....	.....	.....	06 20	07 34						
	86	.....	.....	.....	14 51	15 53						
		39.75		29.83; Index + 0.32; Reduction to 30° = 0.03; = 30.12.					567.705	+0.35	+1.58	86314.95

A bottle was then carefully filled with the hydrogen gas from the apparatus by Mr. NEWMAN, and conveyed by him to Mr. FARADAY at the Royal Institution for examination, from whom I have since received the following note. "I have examined the hydrogen gas, and find no appreciable quantity of air in it. If it contains any, it is less than $\frac{1}{100}$th part; and this I think will be as nothing in your experiments."

The remainder of the hydrogen was then pumped out of the apparatus to

the extent that the state of the pump would permit, which left the gauge at 0.8 inch: when the following observations were made.

Observer.	No. of Coincid.	Thermometers. 1	2	3	4	Gauge.	Times of Disapp.	Reapp.	Coincidences.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 30°.	Corrected Vibrations at 30°.
		°	°	°	°	Inch.	m s	m s	h m s	Div.	s	s	s	
Mr. Taylor.	1						53 24	53 36						
	2	39.8	40.2	40.3	40.4	0.86	1 49	2 01	1 01 55.07	1.05 = 1.22				
	3	.....	.....	.....	.....	.....	10 16	10 28			507.66	+1.76	+1.94	86316.69
	44						57 07	57 27						Gauge 0.82
	45	39.9	40.4	40.3	40.8	0.84	05 35	05 44	7 05 44.83	0.715 = 0.86				
	46	.....	.....	.....	.....	.....	14 03	14 23			509.284	+0.82	+2.00	86316.90
	88	.....	.....	.....	.....	.....	10 24	11 05						Gauge 0.86
	89	40.1	40.6	40.5	40.8	0.88	18 55	19 28	13 19 13.33	0.47 = 0.56				
	90	.....	.....	.....	.....	.....	27 26	28 02			510.37	+0.25	+2.05	86317.12
	150	.....	.....	.....	.....	.....	57 46	58 30						Gauge 0.925
	151	40.1	40.6	40.6	40.8	0.87	6 17	6 54	22 06 26.67	0.69 = 0.24				
	152	.....	.....	.....	.....	.....	14 50	15 23						
		39.97; +0.75 = 40.72				0.872								86316.90

A free communication was then established with the external air, and the following observations made.

Observer.	No. of Coincid.	Thermometers. 3	4	Reduced Barom.	Times of Disapp.	Reapp.	Coincidences.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 30°.	Corrected Vibrations at 30°.
		°	°	Inch.	m s	m s	h m s	Div.	s	s	s	
Mr. Taylor.	1				59 33	59 46						
	2	41.2	41.0	30.14	00 18	00 38	0 0 24.5	0.91 = 0.97				
	3	.....	.....	.....	08 34	08 46			494.96	+0.56	+2.11	86306.39
	24				1 20	1 55						
	25	41.3	41.6	30.11	9 34	10 10	3 9 52.5	0.225 = 0.27				
	26	.....	.....	.....	17 53	18 23						
		41.25		30.125; Capill. + 0.019; Reduction to 32° − 0.031; = 30.113.								86306.39

The vibrations in this experiment were as follows:

In an atmosphere of hydrogen gas . . . . . 86314.95; Barom. 30.120 inches.

In rarefied hydrogen gas . . . . . 86316.90; Gauge 0.872

In atmospheric air . . . . . 86306.39; Barom. 30.113 inches.

Whence it appears, 1st, that at nearly equal heights of the barometer (30.120 inches for the hydrogen gas, and 30.113 inches for the atmospheric air) the pendulum made 8.56 vibrations per diem more in hydrogen gas than in atmospheric air; the temperature of the gas being 39°.75, and of the air 41°.25. And 2nd, that the pendulum made 1.95 vibration per diem more in hydrogen gas, when the height of the gauge was reduced to 0.872 inch, than when the

pressure of the gas was 30.120 inches: being a difference of 1.95 vibration per diem, corresponding to a barometric difference of 29.248 inch. of hydrogen gas: the temperature of the gas being 39°.75 in the compressed, and 40.72 in the rarefied state.

We have in this and the preceding experiment, data for deductions on three distinct points: 1st, on the retardation occasioned by an atmosphere of hydrogen gas: 2nd, on the retardation occasioned by an atmosphere of common air: and 3rd, on the comparative retardation in air and in hydrogen gas.

1st. From the results of Exp. VIII, we have 1.95 vibr. per diem, corresponding to 29.248 inches of the barometer of hydrogen gas; whence 2 vibrations per diem is the reduction to a vacuum for hydrogen gas of 40° under 30 inches pressure: and the number of vibrations per diem of the pendulum in a vacuum, derived from the vibrations in hydrogen gas, is 86316.95.

2nd. We have the vibrations in a vacuum 86316.95, — the number in atmospheric air 86306.39, = 10.56 vibrations per diem; which is therefore the reduction to a vacuum for 30.113 inches of air at 41°.25.

3rd. We have the ratio of the retardations occasioned by air and hydrogen gas, both at 40° and under a pressure of 30 inches, as 10.55 : 2.

Combining the VIIth and VIIIth Experiments, we have corroborative results on the 2nd and 3rd points, from the vibrations in air and hydrogen gas on the 9th and 10th of February. The pendulum in Exp. VII. made 86314.085 vibrations in hydrogen gas of 39°.32 under 30.192 inches pressure; equivalent to 86314.085 + 2.01 = 86316.095 in a vacuum. The vibrations in atmospheric air in the same experiment were 86305.57; Barom. 30.193; and temperature of air 38.1: whence the reduction to a vacuum for air of that temperature, and under that pressure, is 86316.095 — 86305.57 = 10.525 vibrations per diem. And the ratio of the retardations in air and in hydrogen gas, both at 40°, and under 30 inches barometric pressure, is as 10.41 : 2.

Bringing together the two results in regard to this ratio, we have 10.55 : 2; and 10.41 : 2. Or generally, the retardation in air, is to that in hydrogen gas, as $5\frac{1}{4}$: 1. Now the ratio of the densities of air and hydrogen gas being about as 13 : 1, if the resistance of the elastic fluids to bodies falling through them were simply as the respective densities of the fluids, the retardation occasioned by air should be 13 times as great as that occasioned by hydrogen gas. The difference of this ratio from that shown by experiment is greater than can well be

ascribed to accidental error in the experiment, particularly as repetition produced results almost identical. May it not indicate an inherent property in the elastic fluids, analogous to that of viscidity in liquids, of resistance to the motion of bodies passing through them, independently of their density? a property, in such case, possessed by air and hydrogen gas in very different degrees; since it would appear from the experiments, that the ratio of the resistance of hydrogen gas to that of air is more than double the ratio following from their densities. Should the existence of such a distinct property of resistance, varying in the different elastic fluids, be confirmed by experiments now in progress with other gases, an apparatus more suitable than the present to investigate the ratio in which it is possessed by them, could scarcely be devised: and the pendulum, in addition to its many important and useful purposes in general physics, may find an application for its very delicate, but, with due precaution, not more delicate than certain, determinations, in the domain of chemistry.

Experiments IX, X, XI.

These experiments are classed together, their object being the same, and distinct from any of the preceding. It yet remained to be established by experiment, that with a free communication between the interior of the apparatus and the external air, the pendulum, vibrating within the glasses, made the same number of vibrations as if the glasses had not been present. For this purpose the foot screws of the apparatus were simultaneously lowered, so as to detach the upper of the three middle glasses from the suspension piece. The glasses could then be removed, and replaced, in successive observations; the apparatus being in the same state, with the glasses replaced, as in the observation in air in the preceding experiments, with the exception of a disjunction of less than the tenth of an inch between the upper glass and the suspension plate.

Exp. IX. Feb. 17th.—Clock gaining 4m 14s.38. Observer Mr. Taylor.

	No. of Coincid.	Thermometers. 1	Thermometers. 4	Standard Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 32°.	Corrected Vibrations at 32°.
Receiver removed. Vibrations in free air.	1	°	°	inch.	m s 53 09	m s 53 15	h m s 23 1 21.67	Dif. 0.985 = 1.18	s 491.25	s +0.89	s +3.35	86305.82
	2	43.4	43.5	29.70	1 18	1 25						
	3				9 28	9 35						
	23				53 15	53 24						
	24	44.5	44.3	29.71	1 24	1 34	2 1 29.17	0.305 = 0.37				
	25				9 34	9 44						
		43.95		29.705; Capill. + 0.019; Reduction to 32° − 0.039; = 29.685.								86305.82

	No. of Coincid.	Thermometers. 1	Thermometers. 4	Standard Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 36°.	Corrected Vibrations at 36°.
Receivers replaced. Vibrations within the receivers.		°	°	Inch.	m s	m s	h m s	°	s	s	s	
	1				13 00	13 17		Div.				
	2	45.6	45.4	29.72	21 17	21 26	3 21 21.5	0.965 = 1.16				
	3				29 25	29 34			490.85	+0.86	+3.66	86305.81
	23				12 58	13 15						
	24	43.8	44.0	29.76	21 13	21 29	6 21 20.17	0.308 = 0.37				
	25				29 25	29 41						
		44.7		29.74; Capill. + 0.019; Reduction to 32° − 0.040; = 29.719.								86305.81
Receivers removed. Vibrations in free air.		°	°	Inch.	m s	m s	h m s	°	s	s	s	
	1				18 59	19 07		Div.				
	2	43.6	43.7	29.77	27 06	27 15	7 27 10.83	0.99 = 1.19				
	3				35 15	35 23			490.86	+0.93	+2.86	86305.12
	23				18 49	19 04						
	24	42.0	42.0	29.79	27 03	27 16	10 27 09.67	0.345 = 0.41				
	25				35 16	35 30						
		42.8		29.78; Capill. + 0.019; Reduction to 32° − 0.036; = 29.763.								86305.12

In free air; commencing series . . . 86305.82; Barom. 29.685; Therm. 43°.95
——— concluding series . . . . . 86305.12; Barom. 29.763; Therm. 42 .8

Mean . . . 86305.47; Barom. 29.724; Therm. 43 .37

Within the glasses. . . . . 86305.81; Barom. 29.719; Therm. 44 .7

Whence an excess of 0.34 vibr. per diem within the glasses.

Exp. X.—Febr. 21st. Clock gaining 4m 14s.38. Observer Mr. Taylor.

	No. of Coincid.	Thermometers. 1	Thermometers. 4	Standard Barom.	Times of Disapp.	Times of Re-app.	Times of Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 36°.	Corrected Vibrations at 36°.
Receivers removed. Vibrations in free air.		°	°	Inch.	m s	m s	h m s	°	s	s	s	
	1				8 14	8 21		Div.				
	2	43.9	44.0	29.06	16 24	16 31	22 16 27.83	0.965 = 1.16				
	3				24 35	24 42			491.93	+0.91	+3.47	86306.46
	23				8 29	8 44						
	24	44.6	44.4	29.03	16 42	16 58	1 16 50.33	0.33 = 0.40				
	25				24 56	25 13						
		44.25		29.045; Capill. + 0.019; Reduction to 32° − 0.038; = 29.026								86306.46
Receivers replaced. Vibrations within the receivers.		°	°	Inch.	m s	m s	h m s	°	s	s	s	
	1				58 24	58 33		Div.				
	2	46.6	46.4	29.02	6 33	6 42	2 6 37.33	0.92 = 1.10				
	3				14 42	14 50			490.90	+0.79	+4.18	86306.31
	23				58 15	58 36						
	24	45.3	45.3	29.00	6 27	6 47	5 6 37.17	0.295 = 0.35				
	25				14 38	15 00						
		45.95		29.01; Capill. + 0.019; Reduction to 32° − 0.043; = 28.986.								86306.31
Receivers removed. Vibrations in free air.		°	°	Inch.	m s	m s	h m s	°	s	s	s	
	1				50 16	50 27		Div.				
	2	46.3	46.5	29.00	58 26	58 36	5 58 31.00	0.96 = 1.15				
	3				6 35	6 46			491.13	+0.87	+4.08	86306.47
	23				50 13	50 33						
	24	45.1	45.2	29.98	58 25	58 47	8 58 35.83	0.31 = 1.37				
	25				6 37	7 00						
		45.6		28.99; Capill. + 0.019; Reduction to 32° − 0.042; = 28.967.								86306.47

The vibrations in this experiment are as follows:

In free air, the commencing series 86306.46; Barom. 29.026; Therm. 44°.25
——— the concluding series . 86306.47; Barom. 28.967; Therm. 45 .7

Mean . . . 86306.46; Barom. 28.996; Therm. 44 .97

In air within the receivers . . . . . 86306.31; Barom. 28.986; Therm. 45 .95

Whence the vibrations within the receivers are in this experiment in defect, 0.15 per diem.

Exp. XI.—February 22nd. Clock gaining $4^m\ 14^s.38$. Observer Mr. Taylor.

	No. of Colncid.	Thermometers. 1	Thermometers. 4	Standard Barom.	Times of Disapp.	Times of Re-app.	Coincidence.	Arc registered and true Arc.	Mean Interval.	Correction for Arc.	Reduct. to 30°.	Corrected Vibrations at 30°.
Receivers removed. Vibrations in free air.		°	°	inch.	m s	m s						
	1	.....	.....	.....	37 16	37 26		Div.				
	2	44.6	44.7	29.03	45 26	45 37	h m s 21 45 31.33	0.92 = 1.10				
	3	.....	.....	.....	53 36	53 47			491.92	+0.79	+3.66	86306.53
	23	.....	.....	.....	37 28	37 50						
	24	44.8	44.7	29.06	45 43	46 05	0 45 53.83	0.29 = 0.35				
	25	.....	.....	.....	53 58	54 19						
		44.7		29.055; Capill. + 0.019; Reduction to 32° − 0.040; = 29.034.								86306.53
Receivers replaced. Vibrations within the receivers.		°	°	inch.	m s	m s						
	1	.....	.....	.....	58 28	58 41		Div.				
	2	45.0	44.8	29.10	6 38	6 52	h m s 2 6 44.5	0.90 = 1.08				
	3	.....	.....	.....	14 47	15 01			491.85	+0.76	+3.52	86306.30
	23	.....	.....	.....	58 38	59 08						
	24	43.8	43.7	29.16	6 52	7 16	5 7 05.17	0.28 = 0.34				
	25	.....	.....	.....	15 06	15 31						
		44.4		29.13; Capill. + 0,019; Reduction to 32° − 0.039; = 29.110.								86306.30
Receivers removed. Vibrations in free air.		°	°	inch.	m s	m s						
	1	.....	.....	.....	57 22	57 31		Div.				
	2	43.5	43.6	29.17	5 31	5 42	h m s 6 5 36.83	0.93 = 1.12				
	3	.....	.....	.....	13 42	13 53			492.8	+0.89	+2.90	86306.49
	23	.....	.....	.....	57 52	58 20						
	24	42.3	42.5	29.22	6 06	6 32	9 6 18.5	0.32 = 0.38				
	25	.....	.....	.....	14 17	14 44						
		42.9		29.195; Capill. + 0.019; Reduction to 32° − 0.035; = 29.179.								86306.49

The vibrations in this experiment are:

In free air, the commencing series . 86306.53; Barom. 29.034; Therm. 44°.7
——— the concluding series . 86306.49; Barom. 29.179; Therm. 42 .9

Mean . . 86306.51; Barom. 29.106; Therm. 43 .8

In air within the receivers . . . 86306.30; Barom. 29.110; Therm. 44 .4

Whence the vibrations within the receivers are in this experiment in defect, 0.21 per diem.

Collecting the three results in one view, they are as follows:

Feb. 17. Exp. IX. The vibrations within the glasses in excess 0.34
Feb. 21. Exp. X. The vibrations within the glasses in defect 0.15
Feb. 22. Exp. XI. The vibrations within the glasses in defect 0.21

Mean. The vibrations within the glasses in defect 0.007 per diem.

We may therefore conclude that the vibration in air within the glasses, and in the free air of the apartment, the glasses being removed, will lead by sufficient repetition to an identical result.

Finally, the knife edge being examined, was found apparently as clean and sharp as when first used. The agate planes had retained their horizontality; and the screws securing the planes to the circular plate, and the plate to the iron frame, were as tight as when the experiments were commenced.

We have now to collect in one view the several results, from whence the reduction to a vacuum for an invariable pendulum vibrating in air is to be derived.

		Vibrations; corresponding to inches of mercury at 38° of air at		
Exp. I. June 1828. London	7.38	22.765	72.01
II. July 1828. London	9.065	27.721	69.70
III. Jan. 1829. Greenwich	9.54	27.459	35.89
IV. Jan. 1829. Greenwich	9.17	26.138	32.37
V. Jan. 1829. Greenwich	10.38	29.109	36.40
VI. Jan. 1829. Greenwich	5.705	16.080	35.65
VII. Feb. 1829. Greenwich	10.525	30.193	38.10
VIII. Feb. 1829. Greenwich	10.560	30.113	41.25
Mean	9.042	26.197	45.17

Whence we obtain 10.36 vibrations per diem, as the reduction to a vacuum of the invariable pendulum, vibrating in air of 45°, under a pressure of 30 inches of mercury at 32°.

To exhibit the degree of approximation, with which the result of each of the experiments, combined in producing this mean determination, is represented by it, we may compute the several retardations corresponding to the circumstances of each experiment, and place the computed retardations in comparison with the results of actual observation.

	Vibrations.		Vibrations.		Vibrations.
Exp. I.	Computed 7.42 ;	Observed 7.38 ;	Computed + or − ;		+ 0.04
II.	——— 9.08 ;	——— 9.065 ;	———		+ 0.015
III.	——— 9.66 ;	——— 9.54 ;	———		+ 0.12
IV.	——— 9.26 ;	——— 9.17 ;	———		+ 0.09
V.	——— 10.24 ;	——— 10.38 ;	———		− 0.14
VI.	——— 5.65 ;	——— 5.705 ;	———		− 0.055
VII.	——— 10.575 ;	——— 10.525 ;	———		+ 0.05
VIII.	——— 10.48 ;	——— 10.56 ;	———		− 0.08

Hence we may perceive, that were the reduction to a vacuum separately derived from each of the eight experiments, it would in no instance differ more than 0.14 of a vibration from the adopted determination. In other words, the greatest difference that would be occasioned, by deriving the reduction, which it was the object of these experiments to obtain, from any single experiment, instead of from the mean of the whole, would in no case exceed $\frac{1}{75}$th part of the amount of the reduction.

The "correction for buoyancy," or the reduction that would have been previously computed, for the vibrations of a pendulum in air of 45°, under a pressure of 30 inches, is 6.26 vibrations per diem. The actual retardation is therefore 4.1 vibrations per diem greater than had been supposed; and the proportion, which the experimental reduction bears to that which is now shown to have been erroneous, is as 1.655 to 1.

In considering the modifications, which the substitution of the true for the erroneous reduction to a vacuum will introduce, in the results obtained with

invariable pendulums on the variation of gravity at different parts of the earth's surface, we may remark, in the first place, that such results, being merely relative, are not liable to more than a very small proportion of those considerable derangements, in which all determinations hitherto made of the absolute length of the pendulum are involved. The error to which the relative results of invariable pendulums are liable, is limited, in all cases, to a function of the difference in the amount of the buoyancy at different stations, caused by variations in the atmospheric circumstances. With pendulums of the form and materials of those used in the present experiments, we obtain from the results, 0.65, as the co-efficient of the difference; or in other words, the error to which the results are liable is about two-thirds of the difference in the amount of the correction for buoyancy computed for the different stations. The proportion of this error, occasioned by barometric variations, cannot be otherwise than extremely small, in all cases of comparison between stations little removed from the level of the sea. The specific gravity of the pendulum being about 8.6, an inch in the height of the barometer will correspond in buoyancy to about .21 of a vibration a day, which multiplied by 0.65 is about 0.14 of a vibration. In the comparison of tropical and extra-tropical stations, the barometer in the middle and high latitudes is liable to fluctuate an inch, and even in extreme cases more than an inch, from the mean height, which is uniform, or nearly so within the tropics: but as the observations generally include several days at each station, and as in proportion to their continuance the barometer will approximate to its mean height, it will be found, on consulting the record of pendulum experiments, that a difference of half an inch in the barometric height at two stations is a rare occurrence. The correction for half an inch is not more than 0.07 of a vibration, to be added to the number of daily vibrations at the station where the barometer was highest. The liability to error from variations of temperature at different stations is, however, far more considerable than from variations of the barometer: sufficiently so, indeed, to become, in some cases, influential on the ellipticity deduced. A difference of 40° of FAHRENHEIT is by no means of rare occurrence between the tropics and the high latitudes; and as 16° of FAHR. are equivalent, in their influence on the density of the air, to one inch of the barometer, the error in such case may amount to $0.52 \times 0.65 = 0.34$ of a vibration per diem. Moreover, as the

difference of temperature is always in favour of the tropical stations, the error will be of a constant nature, unlike the greater part of the small irregularities to which pendulum experiments are liable, which may compensate themselves by the multiplication of the experiments.

The stations which I have myself visited with the pendulum embrace a greater range and variety of temperature, I believe, than those of any other experimentalist. The preceding remarks would therefore apply particularly to them, but for a circumstance which has fortunately occasioned, in all cases, a compensation of the errors, which would otherwise have arisen from the influence of the variations of temperature on the density of the air. This compensation is a consequence of the "correction for temperature," (i. e. the correction of the vibrations for the temperature of the pendulum,) having been obtained by the peculiarly practical mode, of vibrating the pendulum in London in temperatures differing so widely as to include the whole range experienced elsewhere,—instead of deriving the correction from the expansion of the metal in pyrometric experiments. Had the vibrations at high and low temperatures in London, from which the correction for temperature was obtained, been reduced to a vacuum by the true reduction, as it is now known,—instead of by the "correction for buoyancy," which was then thought to constitute the true reduction,—the value of a degree of Fahr. on the daily vibration would have been found 0.43 instead of 0.421. The first of these numbers is the true correction for temperature due to the expansion of the metal; the second is that correction diminished by the effect of a degree of temperature on the part of the reduction to a vacuum heretofore neglected. It is the second number (0.421) which has been used throughout the experiments to which I allude, in reducing them to a mean term of comparison; consequently the corrections for temperature so applied are every where too small, if regarded as representing only the effect of the expansion of the metal on the vibrations of the pendulum; but they are experimentally correct, when regarded as representing the joint effects of the expansion of the metal, and of the difference of temperature on the part of the reduction to a vacuum not comprehended in the correction for buoyancy.

The effect of differences of barometric height on the results of those experiments is too inconsiderable to require express correction; not exceeding 0.02

of a vibration in the mean acceleration of the pendulum derived from the comparison of the tropical and extra-tropical stations.

The recalculation of those experiments, therefore, with the more correct elements of reduction that we now possess, would have no other effect than that of adding an equal amount to the number of vibrations of the experimental pendulum at every station: or an amount so nearly equal, that the difference is wholly inconsequential: leaving the acceleration of the pendulum at the different stations, which was the object of research, as already deduced.

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METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

1828. July.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in Inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♂ 1	29.951	76.4	29.933	74.9	61	72.7	70.8	63.3	74.8		SSW	Fine—partially lowering.
☿ 2	29.909	71.6	29.895	74.3	65	67.4	74.2	65.0	75.4		SSW	A.M. Light rain. P.M. Fine and clear —light clouds.
♃ 3	29.935	72.8	29.922	74.8	63	70.3	76.8	65.2	78.7		S	A.M. Lowering. P.M. Fine and clear, light breeze. Distant thunder at night.
♀ 4	29.865	73.4	29.899	76.7	70	72.7	74.3	67.6	77.5	0.750	SW	A.M. Fine.—3h to 5h frequent thunder and lightning with heavy rain. P.M. Overcast.
♄ 5	29.908	73.3	29.875	76.2	59	72.7	74.9	65.2	77.3		SW	Fine and clear—light clouds.
☉ 6	29.884	74.4	29.894	74.4	59	69.5	70.2	60.8	73.7		WSW	Fine and clear—cloudy.
☽ 7	29.900	69.3	29.859	73.3	60	64.7	70.8	58.3	74.7		SSE	Clear—cloudy.
♂ 8	29.705	72.0	29.641	75.0	71	71.4	77.7	61.8	79.6		ENE	Fine—cloudy.
☿ 9	29.545	67.8	29.583	69.8	66	66.0	68.4	63.6	67.8	0.086	NW	Dark and overcast. Heavy showers. Distant thunder.
♃ 10	29.688	63.1	29.817	69.3	56	60.0	70.9	56.7	72.7	0.833	NNE	A.M. Showery. P.M. Fine and clear.
♀ 11	29.984	68.3	29.928	70.6	61	66.7	69.8	60.8	73.1		SSW	Lowering.
● ♄ 12	29.532	65.5	29.499	66.2	62	62.2	61.4	60.8	64.5	0.055	NW	Overcast—light fog. Heavy showers.
☉ 13	29.442	61.7	29.497	65.7	57	57.3	63.7	55.4	65.2	0.361	WSW	Brisk wind. A.M. Rain. P.M. Cloudy. Evening fine and clear.
☽ 14	29.563	70.6	29.521	66.8	51	64.0	65.6	52.9	68.5	0.017	WSW	Fine and clear. Heavy showers.
♂ 15	29.489	65.4	29.534	67.3	61	62.3	65.8	55.6	68.3	0.250	S	A.M. Lowering. P.M. Fine. Thunder, with heavy showers, at 1 P.M.
☿ 16	29.768	61.3	29.806	68.8	59	59.0	73.7	53.7	74.8	0.175	NNW	Fine—cloudy.
♃ 17	29.702	66.9	29.691	70.3	63	65.6	72.6	59.5	74.2	0.036	SSW	A.M. Cloudy—brisk wind. P.M. Fine, and nearly cloudless.
♀ 18	29.598	67.7	29.571	70.0	65	65.7	68.6	62.3	72.2		ESE	Cloudy and showery. Evening fine.
♄ 19	29.565	69.0	29.601	70.3	57	67.7	69.6	59.3	71.6	0.039	WSW	Fine and very clear—light breeze.
☉ 20	29.417	64.7	29.354	67.7	60	62.3	66.1	56.8	66.8		ESE	Cloudy—light wind. Heavy rain. Thunder & lightning at 4h 30m P.M.
☽ 21	29.535	70.7	29.541	69.3	57	66.4	68.3	54.8	71.2	1.167	WSW	Fine and clear—light diffused clouds —brisk wind. Heavy showers.
♂ 22	29.551	64.7	29.615	69.2	57	62.9	69.4	52.8	71.6	0.664	WSW	Fine and clear—cloudy.
☿ 23	29.739	70.7	29.706	70.2	61	67.7	70.8	58.7	72.7	0.067	WSW	Fine—cloudy. Heavy rain P.M.
♃ 24	29.659	65.9	29.615	68.8	62	62.3	66.0	59.8	70.7	0.300	SSW	Cloudy—light fog. Showers.
♀ 25	29.580	69.8	29.588	69.8	59	68.0	67.7	60.6	72.8	0.167	SW	Fine & clear.—Showers & brisk wind.
○ ♄ 26	29.767	70.8	29.745	70.2	59	69.2	67.7	57.4	70.6	0.122	WNW	Fine—cloudy. Showers.
☉ 27	29.797	70.7	29.832	68.8	57	64.8	66.4	54.8	68.3	0.058	S	Fine—cloudy.
☽ 28	30.013	62.2	30.016	66.9	52	58.7	65.0	54.5	67.3		N	Brisk wind. A.M. Cloudy. P.M. Fine.
♂ 29	29.991	67.3	29.868	66.7	50	62.5	64.0	54.2	67.6		NNW	Fine and clear—light wind.
☿ 30	29.923	63.6	29.952	64.8	49	59.8	61.9	46.8	63.8		NNW	A.M. Fine & cloudless. P.M. Overcast.
♃ 31	30.036	60.6	30.022	64.5	50	58.7	65.3	48.8	67.5		W	Fine—light clouds.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.737	68.3	29.733	70.0	59.3	65.2	68.9	58.3	71.5	4.147		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.648 } { 3 P.M. 29.640 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1828. August.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock P.M. Barom.	3 o'clock P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Rised off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♀ 1	29.938	64.5	29.954	66.5	53	64.1	66.1	56.7	68.8	0.075	W	Fine—light clouds—brisk wind.
♄ 2	29.738	63.8	29.674	65.4	61	63.2	61.9	58.8	65.0		SSW	Dark & cloudy—strong wind. Heavy showers.
☉ 3	29.673	67.2	29.573	68.5	58	62.9	67.1	56.3	70.4	0.125	SW	A.M. Fine—brisk wind. P.M. Heavy showers.
☽ 4	29.588	66.0	29.582	67.6	58	63.4	68.4	55.7	69.7	0.496	W	Fine—light clouds.
♂ 5	29.638	67.2	29.633	69.1	54	64.1	69.3	54.5	72.9	0.100	WNW	Fine and clear—light wind.
☿ 6	29.489	63.7	29.386	66.2	57	61.8	63.1	56.9	66.5		SSE	Light rain continued. Thunder and lightning at 7h 10m P.M.
♃ 7	29.459	67.5	29.468	69.4	59	66.1	69.2	57.7	71.7	0.291	SW	Fine—showery. Heavy rain at 8h P.M.
♀ 8	29.649	68.8	29.649	69.7	62	67.6	70.5	59.8	71.9	0.321	SW	Fine—light clouds—brisk wind.
♄ 9	29.470	68.2	29.426	68.7	64	65.9	64.4	59.3	67.8	0.094	SSW var.	Strong unsteady wind. A.M. Fine. P.M. Overcast.
● ☉ 10	29.658	68.7	29.656	68.5	53	66.3	67.4	56.7	69.3		SSW	Fine & clear—showery. Strong wind.
☽ 11	29.796	67.7	29.699	67.6	54	65.3	67.9	56.1	68.4	0.035	SW	Fine—light clouds. Evening, rain with strong wind.
♂ 12	29.822	65.8	29.843	68.4	61	63.4	68.7	56.3	69.3	0.258	W	Fine—light clouds. P.M. Showery.
☿ 13	29.834	63.2	29.754	65.7	60	59.4	61.8	54.5	66.8	0.025	NW	Cloudy—continued rain.
♃ 14	29.535	59.9	29.557	60.2	56	56.4	55.8	55.2	56.9	0.988	NE	Strong fog—brisk wind.
♀ 15	29.858	57.9	29.910	63.0	50	56.4	61.7	50.1	63.9	0.063	NW	Cloudy. Evening fine and clear.
♄ 16	29.973	63.4	29.956	62.8	55	55.1	56.3	50.3	67.6		W	Fine—cloudy.
☉ 17	29.909	63.6	29.874	66.3	61	64.2	67.1	55.1	69.5		SW	Cloudy—brisk wind. Light showers.
☽ 18	29.942	64.7	29.965	67.5	58	62.9	67.3	54.4	68.9		WSW	Fine and clear—cloudy.
♂ 19	30.136	60.7	30.157	66.1	57	60.1	68.2	53.6	69.8		W	Fine and clear—light clouds.
☿ 20	30.109	63.2	30.024	66.5	59	62.7	69.2	54.9	70.3		S	Fine and clear—light clouds.
♃ 21	29.876	64.9	29.808	66.7	57	63.3	63.5	58.0	65.9		SW	A.M. Fine. P.M. Lowering—light rain.
♀ 22	29.794	60.7	29.736	64.3	59	58.6	63.1	53.1	64.3	0.035	WSW	Fine—cloudy. Showers.
♄ 23	30.047	60.4	30.103	64.8	57	59.8	65.1	52.0	66.1	0.086	NW	A.M. Fine and clear. P.M. Overcast.
☉ 24	30.211	59.3	30.196	65.5	53	60.4	70.2	49.8	71.8		W	Fine—nearly cloudless.
○ ☽ 25	30.315	66.9	30.313	70.3	66	67.5	72.2	60.9	74.5		NW	Overcast.
♂ 26	30.355	64.8	30.328	68.7	63	62.9	69.5	58.8	70.9		SE	A.M. Overcast. P.M. Fine & cloudless.
☿ 27	30.315	63.1	30.280	67.1	62	63.5	69.2	54.0	70.7		E	Fine and cloudless.
♃ 28	30.225	64.4	30.200	68.6	64	63.6	71.7	57.9	72.7		E	Cloudless and very clear.
♀ 29	30.202	64.5	30.184	69.1	63	63.2	69.8	57.6	71.4		NNE	A.M. Cloudy—brisk wind. P.M. Fine.
♄ 30	30.196	65.1	30.170	69.8	64	63.5	67.9	59.8	70.5		NNE	Fine—cloudy.
☉ 31	30.113	62.8	30.041	64.7	58	60.1	61.8	56.9	62.2		NNE	Lowering—brisk wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.894	64.3	29.874	66.9	58.7	62.5	66.6	55.9	68.6	2.942		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.813 | 3 P.M. 29.780 }

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Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

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1828. Septemb.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock P.M. Barom.	3 o'clock P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☽ 1	30.011	61.9	29.982	65.5	59	60.0	65.2	57.0	68.1		NE	A.M. Lowering. P.M. Fine—cloudy.
♂ 2	30.049	62.8	30.047	66.0	56	61.3	66.1	54.9	67.6		NE	A.M. Fine and clear—light breeze. P.M. Overcast.
☿ 3	30.077	59.5	30.075	64.4	57	57.7	64.1	54.2	64.6		NE	Dark and cloudy.
♃ 4	30.065	62.1	30.012	65.2	55	61.5	64.4	55.9	68.1	0.005	NE	A.M. Fine and clear. P.M. Overcast.
♀ 5	30.023	60.7	30.010	64.0	58	58.5	64.3	54.4	65.4		NE	Fine—cloudy.
♄ 6	30.060	58.9	30.053	64.8	58	58.0	63.1	51.6	66.1		E	Fine and clear—light clouds.
☉ 7	30.049	64.6	30.039	67.2	61	64.4	70.1	58.1	73.0		SE	Fine and very clear—light wind.
☽ 8	29.976	67.2	29.872	69.4	61	67.7	72.1	60.5	73.9		E	Clear & cloudless. Heavy rain at night.
● ♂ 9	29.864	68.3	29.907	70.5	64	66.4	69.9	62.9	72.6	0.141	WSW	A.M. Cloudy. P.M. Fine and clear.
☿ 10	29.802	67.0	29.599	68.1	66	66.2	65.6	57.8	67.9		E	Lowering. Heavy rain P.M.
♃ 11	29.612	67.7	29.598	69.0	64	64.9	67.3	58.9	68.5	0.351	SW	A.M. Fine. P.M. Heavy showers & strong wind.
♀ 12	29.473	66.0	29.431	68.5	64	63.4	67.8	61.3	68.6	0.243	SW	A.M. Heavy rain. P.M. Fine and clear—brisk wind.
♄ 13	29.605	65.2	29.641	67.8	62	62.9	65.9	57.1	67.8	0.121	SW	Fine and clear. Heavy rain in the evening.
☉ 14	29.956	55.6	30.031	59.1	53	53.2	55.4	48.9	56.1	0.216	NE	Lowering. Light rain P.M.
☽ 15	30.304	55.4	29.400	61.2	49	55.0	60.1	48.5	62.7	0.018	N	Fine and clear—light clouds.
♂ 16	30.576	52.8	30.550	58.2	47	51.7	58.9	44.9	59.6		NNE	Fine and clear—light clouds.
☿ 17	30.382	53.9	30.261	59.4	52	54.2	60.2	45.7	61.9		E	Fine and clear—brisk wind.
♃ 18	30.088	58.2	30.014	61.8	54	58.2	62.7	53.4	63.7		E	Fine and cloudless.
♀ 19	30.081	56.1	30.103	61.9	55	55.5	65.8	49.4	66.0		ESE	Clear and cloudless.
♄ 20	30.207	55.8	30.190	62.1	52	55.1	63.8	47.3	65.7		SE	Fine and clear. Strong fog A.M.
☉ 21	30.133	57.1	30.067	62.2	55	56.2	63.1	48.7	64.1		E	Fine and clear.
☽ 22	29.981	58.5	29.964	62.1	57	58.2	65.0	52.1	65.5		SW	Fine—cloudy.
○ ♂ 23	30.128	59.3	30.128	64.4	57	57.8	66.1	54.4	67.0		SW	A.M. Cloudy. P.M. Fine.
☿ 24	30.178	62.2	30.087	65.1	61	62.8	64.9	55.0	67.9		SSW	A.M. Fine. P.M. Overcast.
♃ 25	29.956	64.2	29.891	66.2	61	62.9	68.1	57.8	69.2		SSE	Fine & nearly cloudless—light breeze.
♀ 26	29.810	64.9	29.796	68.1	63	63.8	71.1	58.9	72.8		S	Fine and clear—light clouds.
♄ 27	29.883	60.7	29.829	61.5	57	57.3	59.0	56.2	60.4	0.250	NW	A.M. Heavy rain. P.M. Fine.
☉ 28	29.813	60.1	29.737	64.6	57	58.2	63.7	49.7	64.8	0.285	SSW	A.M. Fine and clear. P.M. Heavy showers.
☽ 29	29.578	63.8	29.537	66.1	62	62.7	65.0	57.6	66.8	0.203	WNW	Lowering—boisterous wind. Heavy rain P.M.
♂ 30	29.708	60.2	29.684	64.2	56	57.8	60.8	52.9	65.4	0.233	WSW	Fine and clear—light clouds.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.980	61.0	29.918	64.6	57.8	59.7	64.7	54.2	66.4	2.066		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.909 | 3 P.M. 29.830 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1828. October.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☿ 1	29.603	58.9	29.629	61.2	55	55.1	58.1	50.8	59.9	0.101	WSW	A.M. Lowering. P.M. Fine & clear.
♃ 2	29.831	53.9	29.879	59.6	51	51.8	59.1	47.1	60.9		WSW	Fine—cloudy.
♀ 3	29.946	50.4	29.861	58.2	47	47.1	58.1	44.2	60.2		SW	Fine and nearly cloudless. Dense fog A.M.
♄ 4	29.556	58.1	29.604	61.5	56	56.7	61.9	46.6	63.3	0.013	S	A.M. Dark and hazy. P.M. Fine—cloudy.
☉ 5	29.607	58.3	29.566	61.2	57	57.4	61.1	51.3	62.7		SSE	Fine—cloudy.
☽ 6	29.364	58.8	29.461	61.3	55	56.7	59.0	52.8	62.2	0.333	SW	Fine and clear—light wind.
♂ 7	29.646	56.6	29.569	60.8	53	54.7	59.8	48.7	60.7		W	Fine and clear—light clouds.
● ☿ 8	29.560	55.7	29.631	59.6	53	54.8	57.7	50.2	59.4	0.011	WSW	Fine—cloudy.
♃ 9	29.994	55.0	30.076	58.8	51	53.5	58.3	47.8	58.7		WNW	Fine—light clouds.
♀ 10	30.163	54.7	30.102	58.3	52	55.3	58.8	46.3	59.3		SW	Fine—cloudy.
♄ 11	30.325	55.4	30.381	59.6	52	53.7	60.3	49.3	61.1		W	A.M. Fine—light haze. P.M. Cloudy.
☉ 12	30.449	56.7	30.449	60.3	55	55.7	62.0	50.3	63.0		SW	Clear and cloudless.
☽ 13	30.415	53.0	30.347	59.4	50	50.7	63.1	47.2	63.4		SW	A.M. Fine—hazy. P.M. Cloudy.
♂ 14	30.381	57.2	30.374	59.6	54	54.7	58.2	50.5	58.5		N	Fine—cloudy.
☿ 15	30.378	52.7	30.329	56.9	50	50.0	54.7	46.0	55.6		NW	Cloudy and hazy.
♃ 16	30.336	54.3	30.297	56.8	50	52.8	55.0	49.7	54.8		N	Hazy.
♀ 17	30.170	54.3	30.118	57.7	52	54.3	57.4	50.4	57.4		WNW	Cloudy.
♄ 18	30.322	50.0	30.310	55.3	40	47.8	52.7	43.8	54.0		NNE	Fine and nearly cloudless—hazy.
☉ 19	30.234	50.0	30.156	53.7	43	49.0	51.7	43.8	51.7		SE	Fine and cloudless—light haze.
☽ 20	30.125	44.3	30.119	50.5	41	42.8	51.9	37.7	51.9		NNE	A.M. Strong haze. P.M. Fine—cloudy.
♂ 21	30.135	50.3	30.085	53.6	47	47.7	55.8	42.2	57.3		S	A.M. Dense fog. P.M. Fine—partially overcast.
☿ 22	29.908	54.7	29.852	58.4	55	57.2	61.0	47.2	62.2		S	A.M. Fine & cloudless. P.M. Cloudy.
○ ♃ 23	29.734	58.4	29.822	56.3	55	55.3	52.7	55.0	55.3	0.014	NNW	A.M. Foggy—light wind. P.M. Rain.
♀ 24	30.152	47.8	30.152	54.2	43	44.8	52.2	40.3	52.2	0.222	WSW	Fine and cloudless—hazy.
♄ 25	30.223	52.4	30.260	55.6	48	50.3	55.0	44.2	55.4		SE	Fine and cloudless.
☉ 26	30.296	47.2	30.296	51.0	41	41.8	51.6	40.5	51.6		S	A.M. Cloudless—strong haze. P.M. Light rain.
☽ 27	30.198	51.6	30.233	54.0	49	50.2	52.6	41.2	53.3	0.028	N	Cloudy and foggy.
♂ 28	30.442	52.3	30.449	53.7	50	51.0	51.0	48.8	51.7	0.055	NNE	A.M. Foggy. P.M. Cloudless—light wind.
☿ 29	30.460	46.3	30.382	51.4	44	44.7	52.8	39.3	52.8		N	Fine and cloudless—hazy.
♃ 30	30.302	46.3	30.288	48.8	41	43.8	47.0	41.3	48.2		NNE	Fine—light clouds—hazy.
♀ 31	30.289	49.3	30.250	47.7	41	42.6	48.3	36.5	48.3		W	A.M. Strong haze. P.M. Fine—light clouds.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.082	52.8	30.073	56.6	49.4	51.1	56.1	46.2	57.0	0.777		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 30.033 } { 3 P.M. 30.014 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

.....above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1828. Novemb.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♄ 1	30.274	48.0	30.222	50.4	46	48.3	51.6	41.8	51.8		WSW	Cloudy and hazy.
☉ 2	30.343	49.2	30.247	51.6	45	48.3	53.3	46.6	53.3		WSW	Cloudy.
☽ 3	30.348	49.6	30.317	52.0	46	46.5	51.7	44.3	51.7		N	A.M. Strong haze. P.M. Fine—cloudy
♂ 4	30.347	46.7	30.196	51.5	44	44.4	50.2	40.7	51.4		NE	Fine.—A.M. Strong haze. P.M. Light clouds.
☿ 5	30.170	46.7	30.162	49.7	42	42.7	48.5	39.3	48.5		ESE	Fine—light clouds.
♃ 6	30.121	47.3	30.082	48.7	45	45.4	48.4	39.6	48.4		ESE	A.M. Fine. P.M. Overcast.
● ♀ 7	30.029	48.7	29.937	49.7	45	45.2	48.4	43.7	47.2		E	Fine and cloudless. Haze and light wind P.M.
♄ 8	29.935	41.1	29.879	41.3	37	36.3	37.6	34.8	38.2		ESE	Overcast—light wind.
☉ 9	29.699	39.6	29.648	43.7	37	37.5	44.4	33.7	44.4		N	Dull—haze and light wind.
☽ 10	29.511	40.3	29.458	41.5	38	37.7	38.8	35.3	39.1		N	Strong fog.
♂ 11	29.550	36.4	29.560	38.3	30	31.6	34.2	29.7	34.2		NW	Strong haze.
☿ 12	29.597	30.8	29.589	35.6	26	27.4	36.7	25.7	40.6		N	Very dense fog.
♃ 13	29.666	37.8	29.661	42.0	41	41.5	49.5	26.3	49.7		S	Fine and clear—light clouds.
♀ 14	29.510	43.8	29.384	45.3	43	44.1	46.9	40.8	48.7		E	Foggy—light rain.
♄ 15	29.351	47.3	29.361	48.8	47	49.4	50.8	43.7	51.5	0.142	E	A.M. Light rain. P.M. Fine & clear.
☉ 16	29.284	50.4	29.227	52.3	51	51.8	53.3	48.9	53.3	0.236	SSE	Showers with boisterous wind.
☽ 17	29.645	50.7	29.718	49.0	48	48.7	49.8	46.8	49.8	0.194	NW	Light clouds—hazy.
♂ 18	29.931	49.3	29.987	50.8	45	45.0	47.0	42.5	47.3		WSW	Fine—hazy.
☿ 19	29.988	48.8	30.144	50.4	45	47.4	48.6	40.8	49.2	0.112	NNW	A.M. Cloudy—light wind. P.M. Fine.
♃ 20	30.143	48.2	30.084	51.7	48	48.6	51.8	43.4	52.3		WNW	Overcast.
○ ♀ 21	29.971	53.0	29.918	54.8	50	52.8	54.7	47.8	55.8		WSW	A.M. Cloudy—light wind. P.M. Fine and clear.
♄ 22	29.860	54.2	29.861	54.9	51	51.8	52.8	48.1	53.7		S	Cloudy. Light rain P.M.
☉ 23	30.013	47.8	29.889	52.3	42	42.8	50.3	40.3	50.7		WSW	Fine and cloudless—light haze.
☽ 24	29.715	51.7	29.729	53.6	49	49.3	51.3	41.9	51.9		SSE	A.M. Fine. P.M. Overcast.
♂ 25	29.929	51.7	29.940	53.9	49	49.2	53.0	45.9	53.5		S	Fine and cloudless—light haze.
☿ 26	29.890	53.8	29.878	55.6	53	53.4	54.8	48.2	55.8		SSW	Cloudy—light wind. Rain P.M.
♃ 27	29.994	52.9	30.097	55.3	47	47.7	52.6	46.3	55.4	0.125	WSW	Fine—light clouds and haze.
♀ 28	30.095	56.4	30.106	58.6	55	55.7	57.5	47.0	58.4		SSW	A.M. Overcast. P.M. Fine.
♄ 29	30.129	56.7	30.102	58.7	54	55.7	56.3	50.7	57.8		SW	A.M. Cloudy. P.M. Fine.
☉ 30	30.146	55.8	30.071	57.1	52	52.4	53.7	49.2	54.4		SW	Overcast.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.899	47.8	29.881	50.0	44.7	46.0	49.2	41.8	49.9	0.809		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.863 } { 3 P.M. 29.839 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about) = 95 feet.

The external Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1828. Decemb.	9 o'clock, A.M. Brom.	Attach. Therm.	3 o'clock, P.M. Barom.	Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☽ 1	29.828	54.3	30.046	49.8	42	49.3	44.5	48.3	48.7		NW	Fine and clear—light wind.
♂ 2	30.455	44.7	30.367	45.4	25	37.4	39.7	33.8	41.7		ESE	Fine—light clouds.
☿ 3	30.170	44.8	30.108	47.3	41	43.0	49·0	36.3	50.0		SSW	A.M. Fine—brisk wind. P.M. Overcast.
♃ 4	30.199	48.8	30.217	50.7	50	50.4	53.1	42.3	53.2		WSW	Light clouds—foggy.
♀ 5	30.185	51.1	30.088	51.8	50	50.7	48.7	48.7	50.7		SSE	Overcast and foggy.
♄ 6	29.932	49.0	29.869	51.2	46	46.5	50.8	40.3	50.8		S	Fine—lightly overcast.
● ☉ 7	29.590	51.7	29.690	52.8	50	50.4	50.8	45.7	51.4	0.036	SSW	Fine and cloudless. Strong wind early A.M. Thunder with rain at night.
☽ 8	29.389	50.4	29.900	50.8	46	46.9	46.7	44.2	47.7	0.231	SW	Cloudy. Thunder and lightning with heavy rain and brisk wind at 1 P.M.
♂ 9	29.668	44.9	29.833	47.8	39	39.8	42.7	37.4	43.7	0.278	SW	Fine and cloudless.
☿ 10	30.160	45.7	30.149	48.0	43	44.3	46.7	37.8	50.0		WSW	Fine and clear—light clouds.
♃ 11	29.953	49.8	30.141	51.8	49	50.7	50.8	43.8	52.3		SSW	A.M. Cloudy. P.M. Fine and clear.
♀ 12	30.388	48.6	30.349	51.7	46	46.4	51.7	41.6	51.7		SW	Overcast and foggy.
♄ 13	30.380	52.3	30.379	53.8	50	50.8	52.6	45.8	52.7		SSW	Overcast and foggy.
☉ 14	30.444	52.0	30.401	52.1	48	48.3	47.0	47.3	48.3		ESE	Fine and cloudless.
☽ 15	30.288	46.3	30.219	47.8	41	41.3	45.0	39.3	47.5		ESE	Overcast and foggy.
♂ 16	30.106	50.0	30.039	51·2	46	48.4	49.2	40.2	51.2		SSE	A.M. Fine—cloudy. P.M. Overcast.
☿ 17	29.856	51.8	29.836	53.2	50	50.2	52.3	47.0	55.0	0.083	SSW	Overcast and foggy. Rain early A.M.
♃ 18	29.446	54.5	29.573	55.3	55	55.3	51.6	49.2	55.3		SSW	A.M. Violent wind early. Heavy rain. P.M. Fine and cloudless.
♀ 19	29.745	50.3	29.878	52.3	45	46.4	49.8	42.0	51.5	0.075	WSW	Fine—light clouds.
♄ 20	29.999	52.8	30.011	54.0	51	52.2	53.4	45.3	53.4		NNW	Overcast.
○ ☉ 21	30.088	54.0	30.071	55.3	52	52.7	55.0	51.3	55.0		WSW	Overcast.
☽ 22	30.190	54.3	30.046	55.8	51	52.3	54.7	50.7	54.7		SW	Overcast.
♂ 23	29.970	53.8	29.884	55.0	51	50.9	51.4	48.8	46.5		SW	Overcast and foggy.
☿ 24	29.676	52.4	29.570	53.8	47	47.7	51.3	46.4	51.3		WSW	Overcast and foggy. Evening rainy.
♃ 25	29.446	49.7	29.484	51.0	45	45.0	45.7	43.7	45.7	0.186	SW	A.M. Foggy. P.M. Fine—lightly overcast.
♀ 26	29.590	45.0	29.561	47.8	38	38.6	43.0	35.8	44.3		S	Fine and cloudless—light haze.
♄ 27	29.689	44.3	29.744	45.8	39	39.0	42.3	36.8	42.3		ESE	Fine and cloudless.
☉ 28	29.887	44.6	29.967	46.3	39	39.8	44.6	38.0	44.6		ESE	A.M. Strong fog. P.M. Overcast—rain.
☽ 29	30.346	44.7	30.382	45.0	41	41.4	40.6	39.3	41.4		ESE	A.M. Strong white fog. P.M. Overcast.
♂ 30	30.377	42.3	30.312	45.9	40	40.7	44.7	34.5	44.7		ESE	Overcast—light fog.
☿ 31	30.064	45.7	29.959	47.0	43	44.4	47.4	39.8	47.7		SSW	Overcast. Light rain P.M.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.981	49.2	29.980	50.6	45.1	46.5	48.3	42.6	49.2	0.889		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.942 } { 3 P.M. 29.936 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... =83 feet 2½ in.

..... above the mean level of the Sea (presumed about)..... =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... =79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.